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A METHODOLOGICAL REVIEW OF EXTRA-SENSORY PERCEPTION[1, 2]

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I. INTRODUCTION

In a recent issue of the *Journal of Parapsychology* (29) the editors make the following statement concerning the need for a comprehensive review of experimental work on extra-sensory perception:[3]

> "One significant feature stands out in the four years of criticism to which the ESP research has been subjected: Up to the present no critic has attempted a thorough and a comprehensive evaluation of the research as a whole . . . The rapid expansion of the explorations in this field makes it particularly urgent that the primary and fundamental question of the occurrence of extra-sensory perception be established once and for all, or the essential weakness of the claim for it be pointed out by a very much more drastic analysis than has yet been forthcoming" (p. 79).

The present review has been designed to meet this need from the standpoint of experimental methodology. The reviewer has attempted to evaluate what he considers to be the important methods, both historical and modern, and to criticize experimental methods with the purpose of improving them. Since the problems of telepathy and clairvoyance have been raised again, he believes that it is rather important that they should be settled, in so far as they are capable of solution, by the application of acceptable scientific methods.

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[1] This paper has been read and approved by the Stanford Committee on Psychical Research.

[2] The assistance of the late Professor John Edgar Coover in compiling this review, given through personal conversations and especially through his complete card index of titles on the history of telepathy and clairvoyance research, is gratefully acknowledged.

[3] Defined in the glossary of common terms found at the end of each number of the *Journal of Parapsychology* as: "Response to an external event (perception) not presented to any known sense." Hereafter referred to as ESP.

II. HISTORICAL SUMMARY OF EXPERIMENTAL METHODS IN THE STUDY OF TELEPATHY AND CLAIRVOYANCE

A. Errors in Experimental Methods

Much of the material to be reviewed under this heading may be found in greater detail in Coover's extensive monograph (**22**), which covers the period from 1882 (establishment of the British Society for Psychical Research) to 1917. Prince (**81**), Bird (**9**, **10**), Murphy (**73**, **74**), and especially Rhine (**83**) have also reviewed experiments on telepathy and clairvoyance during this period and up to the modern work on ESP, which began at Duke University in 1930. Without attempting to evaluate these early experiments with respect to the ESP hypothesis, it is possible to reconsider them from the aspect of their contributions, at the time, to the knowledge of unsatisfactory experimental methods in parapsychological research. For example, the early work of the British Society for Psychical Research served to emphasize the factor of involuntary and unnoticed sensory cues as the basis for some positive results in telepathy experiments. These involuntary cues were utilized in kinesthetic, tactual, auditory, and visual modalities. The work of the British and American Societies also brought about a study of "mental habits," preferences, and "coincidences" as productive of questionable positive results. Finally, some of the recent work of the British Society, although it has not received the critical attention due it, has illustrated the necessity for making independent records of guesses and material-to-be-guessed in telepathy and clairvoyance experiments. Since Richet's (**91**) introduction of methods demanding large numbers of trials in telepathy and clairvoyance experiments, this point has become of some importance.

1. *Minimal and Subliminal Sensory Cues.*

(a) *Kinesthetic and Tactual Cues.* According to Jastrow's recent paper (**51**) on Chevreul's contributions to the history of suggestibility, involuntary muscular movements have been and continue to be associated with supernatural causes. Barrett and Besterman (**4**) made an exhaustive study of "dowsing" or use of the divining rod and reached the conclusion that the movements of the rod were caused by the dowser's own unconscious or involuntary muscular movements. No good evidence for other than normal causes for the movements was obtained. Faraday (**31**) and Chevreul (**21**) are responsible for the definitive studies on involuntary movements as related to " psychic " manifestations. Faraday showed that " table tipping " and " rapping " were caused by unnoticed pressures applied to the table by the sitters; Chevreul, that the swings of a pendulum held in a suggestible person's hand were caused by involuntary movements. Suggestions, even though the subject was not consciously aware of receiving them, determined the direction and amplitude of the swings.

Involuntary movements also served as uncontrolled cues in telepathy experiments. In the middle of the last century, the so-called " willing game " was popular. The sender clasped the receiver's hand or made tactual contact with some part of the body. The receiver then attempted to " read the sender's mind." Sugden (113) gave one of the earliest critical accounts of the successes produced by involuntary movements and suggested that telepathy experiments should be controlled by eliminating tactual contact.

Another variation of the contact cue was investigated by Stratton (105). Stratton's subject, a professional " thought-reader," made a specialty of finding hidden articles. He could be thoroughly blindfolded but a necessary condition for success was found in his use of involuntary kinesthetic cues obtained from a " leader." Another necessary condition for success was found in that the " leader " himself had to know where the object was hidden. The performer was so highly trained at using these involuntary kinesthetic cues that contact through a thin watch chain sufficed to produce remarkable " hits."

When the receiver is allowed to see the sender, involuntary movements or signals may be visually apprehended. Apparently these cues were used by " Clever Hans," the mind-reading horse investigated by Pfungst (76). The visual apprehension of involuntary movements was also proposed by Münsterberg (71) to account for the telepathic feats of Beulah Miller. Her successes decreased to the chance level when the family was outside the room. Some suggestion of the visual use of involuntary muscular cues is to be found in early reports of experiments with the Creery sisters by Barrett, Gurney, and Myers (5). It was noted that the successes of the sisters increased if the father was included in the group of senders and the girls were able to see him. Confessions of fraud (41) further invalidate these experiments, although at one time they were considered to be unimpeachable evidence by members of the British Society for Psychical Research. Button (16, 17, 18) has reported high scores in card guessing with the medium " Margery " as subject. Since, in the majority of the card tests, the sender was in full view of the guesser, the question of involuntary cues appears to be of relevance. It is generally agreed at present that experimental methods which allow contact and use of involuntary muscular cues through vision cannot provide serious evidence for any ability beyond the learned skill in using minimal and subliminal cues to aid guessing.

Probably the best example of the use of tactual cues may be found in the clairvoyance experiment reported by Verall (**117**). She was able to obtain differential tactual cues from feeling the faces of playing cards as she was guessing their denomination. Feeling the backs did not produce positive results.

(b) *Visual Cues.* As stated above, visual observation of involuntary movement may serve to produce positive results in telepathy experiments. Verall (**117**) found that the faces of playing cards could be read with ease if they were dealt out over a polished surface or even over a white linen tablecloth. Coover (**22**) obtained results on the use of subliminal (*i.e.* nonverbalized) visual cues which definitely indicate that subjects may unconsciously use these cues to produce extra-chance results in guessing experiments and cited other experiments in which similar results were found. Since the cues may operate below the level of awareness of the subject, every possible use of visual cues should be eliminated in telepathy and clairvoyance experiments, even though the possibility of using such cues appears to be negligible.

(c) *Auditory Cues.* The error of unconscious or involuntary "whispering" cues in telepathy experiments has received little attention since 1895 when Hansen and Lehmann (**45**) advanced this hypothesis to explain results obtained by the Sidgwicks and G. A. Smith (**99**), members of the British Society for Psychical Research. In the Sidgwick-Smith experiment, extra-chance scores were obtained by hypnotized receivers only so long as the sender was in the same room. Recalculations with modern statistical methods of the results obtained when the sender was outside the room indicate that these scores do not deviate significantly from the chance level. Hansen and Lehmann assumed that hypnotism produced auditory hyperesthesia in the receiver and that the sender, in concentrating, unconsciously whispered enough of the material which he was attempting to "transmit" to raise the total hit score above chance expectancy. In trying to reproduce the conditions of the Sidgwick-Smith experiment, Hansen and Lehmann used 2 parabolic sound reflectors arranged in such a way that involuntary noises made by the agent at the focus of one reflector were reflected and gathered at the focus of the other reflector where the receiver was stationed. Under conditions in which the sender was aware of the purpose of the experiment but reduced his whispering to minimal intensity, scores significantly above the chance level were obtained. Hansen and Lehmann also showed that various positions in a closed room were not equally good for hearing small noises because of the nature of the sound reflection. An analysis of most frequent confusions in the Hansen-Lehmann and the Sidgwick-Smith experiments seemed to indicate to Hansen and Lehmann that errors in both experiments were produced by the same cause: namely, the misinterpretation of reduced and ambiguous sound cues. Sidgwick (**98**), however, pointed out that a good agreement was found between the modal errors in the Hansen-Lehmann experiment and in the trials from the Sidgwick-Smith experiment in which the agent was in another room. Sidgwick also criticized the Hansen-Lehmann work because the whispering was not "involuntary." Kennedy (**60**) has recently repeated the Hansen-Lehmann experiment with naïve senders (blindfolded and unaware of the action of the reflectors) and has found confirmatory evidence for involuntary auditory cues as a source of error in telepathy experiments.

In an unpublished manuscript answering the critics of card-guessing experiments reported in his 1917 monograph, Coover (**23**) claims that the small positive excesses may have been due to the subject's use of involuntary sound cues supplied by the agent. No graded effect of the distance was found between agent and percipient (in the same room) in 847 trials (p. 77), with distances of 1, 2, 3, 4.6, 6, and 10 meters. However, the factor of unconscious whispering in a few agents might have given slight positive deviations when the scores were summed for the total group of 10,000 guesses. Coover's experimental methods were uncontrolled for this source of error.

Finally, in evaluating experiments on telepathy and clairvoyance, due consideration should be given to opportunity for conscious fraud in the experimental conditions. The sequel of the British Society's experiments with the Creery sisters was an exposé of conscious signalling (**41**). The striking successes in the famous Smith-Blackburn series of telepathic experiments (**42**, **43**) were attributed by Blackburn (**12**), 30 years later, to ingenious codes which escaped detection at the time.

2. *"Mental Habits" and Preferences.*

In much of the early work on telepathy, the material to be transferred by thought consisted of simple geometric drawings, familiar objects and incidents. The question soon arose as to how much of the positive evidence for telepathy was attributable to communality of experience and preferences for certain diagrams and orders of diagrams. Pickering (**77**) and Minot (**69**, **70**), members of the American Society for Psychical Research, conducted experiments relating to mental habits and found that percipients in thought-transference situations tended to call numbers and draw diagrams in accordance with their habitual preferences.

The use of simple diagrams and pictures as suitable material for telepathy experiments has also been questioned since the judgment of a "hit" may be dependent upon the experimenter's bias. Minot (**70**), Hall (**44**), and Hansen and Lehmann (**45**) pointed out the unsatisfactory character of such judgments in the early work on telepathy. Sudre (**112**) championed the drawing method and was answered by Bird (**9**), who proposed that in future work the material to be transferred should be simple enough to make possible the application of probability statistics. More recently, the series of telepathy experiments reported by the Sinclairs (**100**), in which simple drawings were used, have revived this issue. Prince (**81**), in a review of the Sinclair experiments, failed to consider the preference factor in discussing the results. Warcollier's (**118**) experimental methods were criticized by Soal (**101**) because the material would not allow exact probability estimations. The preference effects in these experiments arose when the agent was allowed to choose freely or select "mentally" the material to be sent. At present, there appears to be general agreement among experimenters that the material used in telepathy experiments should be chosen for the agent by a method which will insure a random series. Coover's experiments (**22**) illustrate methods and materials in which preference factors were successfully eliminated. Cason (**20**), Troland (**115**), and Estabrooks (**30**) also conducted experiments on telepathic ability in which mental habits and preferences could not produce spurious results. Estabrooks was the only one of this group to obtain extra-chance results in card guessing.

3. *Recording Errors.*

Another important factor in the production of statistical evidence supporting the telepathy and clairvoyance hypotheses has been the lack of control of the human element in recording the results of experiments. Unnoticed errors may occur in the records of individuals who are seeking evidence in support of their beliefs. The necessary control of these errors in recording lies in making independent records of the guesses and the material to be guessed. The recorder should not have knowledge of the correctness or incorrectness of the guess. As will be shown later, this apparently insignificant point of methodology may have many ramifications in explaining positive results in telepathy and clairvoyance experiments.

In the experiments of the Sidgwicks and G. A. Smith (**99**), cited above in connection with the error of involuntary auditory cues, it appears that independent records of guesses and material guessed were not made when the sender and hypnotized receiver were in the same room, *i.e.* under the conditions in which the extra-chance excesses were obtained. Exact details of the recording procedure are not given but it is stated that Mrs. Sidgwick directed and arranged the material to be guessed and did the recording while the principals were in the same room. It may be assumed that independent records of guesses and numbers guessed were made when subject and operator were in different noncommunicating rooms. To complete this error theory, the assumption is further made that the recorder knew what the correct responses should be and may have made unnoticed errors which automatically increased the score above the chance level. Internal evidence that errors in recording were made is to be found in the Sidgwick-Smith paper (Table III, p. 153). Only numbers from 10 to 90 were used in the experiment, yet 91 appears as one of the numbers on which the agent concentrated.

Experimental conditions in the researches of Brugmans (**14**, **15**), in collaboration with Heymans and Weinberg, in which positive results in favor of the telepathic hypothesis were reported, apparently did not exclude the factor of unnoticed errors in recording. The recorder apparently looked through a hole cut in the floor of an upper room and observed the subject making his choices manually by indicating a certain square on a board. If the assumption is made that the recorder knew what the correct choices were, these results are explainable on an error basis. Murphy (**73**) states:

> "Curiously enough, 40% of the experiments between the two rooms were complete successes while only 30% of those in the same room were successful."

If the further assumption is made that peering through a hole at the subject's hand does not constitute a favorable condition for exact observation, this may also have an explanation[4] in terms of recording errors. In an earlier experiment from the Groningen laboratory by Van Loon and Weinberg (116), in which emotions, colors, and thermal sensations were used in attempted thought-transference, the criticism of nonindependent recording also applies as do those of kinesthetic, auditory, and visual cues, and preference effects.

Jephson (52) reported positive results in clairvoyance experiments in which the subject made his own record of both the actual and guessed card orders. Later experiments by the same writer (53) and with Soal and Besterman (54), in which recording was carried out independently, yielded results at the chance level. A telepathy experiment by Besterman (8), in which recording was done independently, yielded negative results for the telepathic hypothesis.

Methods of recording are important to consider in attempting to understand experiments in which results in favor of the telepathic and clairvoyant hypotheses were obtained. In the majority of past experiments, the condition of independent recording reduced the successes to the level expected by chance. An exception to this general rule may be found in Estabrooks' telepathy experiment performed at Harvard University. Many experiments for telepathy and clairvoyance are unsatisfactory because of a combination of such uncontrolled methods; for example, the Sidgwick-Smith series did not eliminate either involuntary auditory cues or unnoticed errors in recording. Coover (22), Cason (20), and Troland (115) have, in the past, suggested that independent recording should be a necessary condition for acceptable scientific methodology in experimentation on telepathy and clairvoyance. The specific ways in which knowledge of correct guesses at the time of recording may produce unnoticed errors will be discussed more fully in connection with the Duke experiments on ESP.

B. STATISTICAL METHODS USED IN PSYCHICAL RESEARCH BEFORE ESP

1. *The Limit of Chance Variation.*

The statistical methods in research on telepathy and clairvoyance have been subjected to criticism since their introduction by Richet (91) in 1884 and their further elaboration by Coover (22) in 1917. Sanger (94) and Edgeworth (27, 28) concluded that the application of theoretical probability formulae to the description of the chance or extra-chance nature of results obtained in simple guessing experiments was justified. Probably the most important criticism concerning statistical methods in pre-ESP telepathy and clairvoyance experiments was raised by Coover's selection of the limit of chance expectancy (**22**). Thouless (**114**) has stated that Coover's limit was too high by a factor of the square root of 2. In explaining his selection of $L = \sqrt{2} \cdot 3\sigma$ as the limit of chance variation, Coover, in a footnote (p. 85), stated that he was following Sanger's (**94**) formula, $K = 3\sqrt{2(1-q)mq}$ [q=probability of occurrence, m=number of trials], which reduces by appropriate substitutions, to the formula used by Coover.

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[4] Murphy (73, 74) has twice written reviews of this experiment without fully stating the types of control exercised beyond guarding against sensory cues. Since the present writer has not been able to obtain the first report, decision as to the probable correctness of the error hypothesis depends upon a further knowledge of the experiment.

2. *Selection of Data.*

When evidence for telepathy and clairvoyance is obtained by comparing frequencies of correct guesses with frequencies expected by chance, one of the more obvious errors in experimental method arises from selection of favorable data for statistical analysis. In the hands of inexperienced experimenters, such selection has in the past produced spurious statistical evidence for the existence of telepathy and clairvoyance. The importance of this source of experimental error led Soal (**102**) to make the following statement:

> "Moreover, instead of being given an exact account of the precise conditions under which each experiment is carried out, we are regaled with 'samples' of successes generally chosen to illustrate preconceived theories of the way in which telepathy is supposed to work. I believe this inveterate determination to find the super-normal at all costs in every series of experiments to be the most injurious influence in psychical research today. The scepticism of the open-minded man of science who has not given much attention to the subject is an entirely wholesome thing compared with the 'will to believe' shown by the class of psychical researchers whose real aim appears to be not the investigation of the conditions under which telepathy and clairvoyance occur, or the question whether they occur at all, but the *production of examples of these faculties for the purpose of bolstering up beliefs they hold on the destiny and spiritual nature of man or for the purpose of confuting the spirit hypothesis.* It is easy to understand how the psychic researcher actuated by such motives becomes the prey to self-deception . . . His desire is for super-normal happenings, and he is not disposed to look too critically at the methods by which these supposedly super-normal facts were established. And experiments in telepathy and clairvoyance unfortunately are the easiest in the world to manipulate so as to give the illusion of success" (pp. 271–272).

In an unpublished manuscript, Coover (**23**) points out that critics, especially Schiller (**95**), who have "recalculated" the results in his 1917 monograph, have obtained their "evidence for lucidity" by selecting scores from a total distribution of trials which exceeded the mean chance expectancy. To quote from this manuscript:

> "Selection for separate treatment should not be made from the figures in the distribution; it should be made from the conditions of the experiment. Or, if selection is made on the basis of figures in the distribution, it should be for the purpose of going back to the data to seek for conditions that may be responsible for the peculiar nature of these figures, rather than for putting them through a statistical mill that is no more applicable to them."

Again:

> "All one needs to do is to select sufficient data removed well from the mean of the distribution and he can 'prove' the presence of any cause he has an interest in promoting."

To prove the Schiller hypothesis of "lucidity in selected cases," consistency measurements on the scores of the selected group would be necessary. Coover did not provide the necessary retest data for the determination of consistency.

In order to summarize briefly this discussion of sources of error found in experiments on telepathy and clairvoyance before ESP, errors and suggested controls may be arranged in the following table:

TABLE I

SOURCES OF ERROR IN PRE-ESP TELEPATHY AND CLAIRVOYANCE EXPERIMENTS

Errors	Controls
1. Minimal and subliminal sensory cues a. Kinesthetic and tactual b. Auditory c. Visual	No contact, preferably distance separating agent and percipient. Vision and audition excluded by suitable *tested* methods.
2. Mental habits and preferences	Use of materials, such as playing cards, with which the probability of success in guessing may be determined. Use of methods for "randomization" of material to be guessed, such as a thorough mechanical shuffle of a deck of cards.
3. Unnoticed errors in recording original data	Independent records. Recording done without knowledge of success or failure.
4. Selection of favorable data	Inclusion of all data *obtained under same conditions* for statistical analysis. Decision as to conditions favorable to occurrence of telepathy and clairvoyance *before* rather than *after* collection of runs.

III. EXPERIMENTAL METHODS IN ESP RESEARCH

A. METHODOLOGICAL ERRORS

The majority of the modern experiments purporting to demonstrate the existence of ESP can be reviewed profitably with respect to the sources of error discussed in the previous section. In the following discussion of the ESP experimental methods, these techniques will be presented by the device of direct quotation from the major publications of the Parapsychology Laboratory and from experimental articles in which positive results obtained by these methods have been accepted as evidence for ESP.[5]

1. *Unnoticed Sensory Cues.*

(a) *Visual Cues.* Three of the methods recommended by Rhine and others are open to the criticism of unconscious use of small visual cues on the part of the ESP subjects. These methods are (1) the Open Matching test, (2) the Blind Matching test, and (3) the Single Card Calling, or Before Touching test.

In the "Handbook," the Open Matching test (OM) is described as follows:

"*Getting ready.* Select five cards, one of each kind, from an extra pack of ESP cards. Place these, as 'key cards,' face up in a row in the middle of the table before you. Lay the rest of the pack aside.

"Thoroughly shuffle another pack of twenty-five ESP cards, and then cut it. Hold it face down in your hand as though you were going to deal the cards.

"*Matching.* Now try to decide which key card the top card of the pack matches. Choose your strongest hunch or clearest impression. (Follow the general directions given for Test I.) When you have decided, remove the top card without looking at it and place it face down in front of that key card.

"Do the same for the second card in the pack, then the third, and so on until all twenty-five cards have been put down where you think they match the key cards.

---

[5] Although *A handbook for testing extra-sensory perception,* written by Stuart and Pratt (**111**), is misleading as to the details of recording in some experimental methods, as will be pointed out later, it will be used as the main source for the general features of Rhine's experimental techniques. In general, the Rhine methodology will be quoted from (1) *A handbook for testing extra-sensory perception,* hereafter referred to as the "Handbook"; (2) Rhine's monograph, *Extra-sensory perception,* hereafter referred to as the "Monograph" [Rhine, **83**]; and (3) experimental articles in the *Journal of Parapsychology,* edited by McDougall and Rhine.

"Place each card as though it were the only one, without regard to where you have put the others. Avoid falling into any system in placing your cards, such as laying them down in order from left to right. You may occasionally choose the same key card twice or three times in succession. Look through a shuffled pack to get an idea of the random order in which cards come. You do not need to keep an even number in each pile nor to have even piles at the end of the run.

"*Checking up.* When you have finished the run, find your score by counting the cards in each pile which match the key card before which they lie. Write the score for the run into one of the spaces of a record sheet.

"*Recording.* If you wish to keep a record of all the cards, list them on the record sheet in the following manner. Record the left-hand key card in the first 'call' space. Record the cards which were placed to match it in the 'card' column. Draw a line under the last card. Then record the other key cards and their corresponding cards in the same manner, separating each pile from the next by a line" (pp. 18–19).

Experience with this method shows that it is practically impossible for the subject to match cards without looking at the backs in the process. A photograph, purporting to illustrate the Open Matching method, may be found opposite p. 246 in Rhine's book (**87**), *New frontiers of the mind.* This photograph shows the subject with the card to be matched in her right hand, looking at the back of the card.

The backs of the cards seem to be available to the subject's vision in the Blind Matching test (BM). Directions for this test in the "Handbook" are as follows:

"This test is like the Open Matching Test except that here the key cards are face down and their order is unknown.

"*Getting ready.* Shuffle the five key cards out of sight so that you do not know their order, and place them face down in a row on the table.

"*Matching.* Take the shuffled pack of ESP cards face down in your hands ready to deal. Try to get a feeling of where the top card should go in order to match a key card. In this test it does not help to attempt to guess the symbols on the key cards. Just place the cards as they seem to belong together. When you have decided where to put the first card, place it down without looking at the face and go on to the next. Keep on until each card has been placed.

"*Checking up.* First turn the key cards face up, then count the number of cards in each pile correctly placed to match the key card. Record your score on one of the record sheets. Or list all the cards as described for Test III on p. 19" (pp. 22–23).

Again, the criticism of possible visual cues from the backs of the cards applies to the Single Card Calling, or Before Touching (BT) test, described in the "Handbook" as follows:

> "*Getting ready.* Place a well-shuffled pack of ESP cards on the table before you. Cut the pack, taking care not to see any of the faces of the cards. The table at which you are working should not have a shiny surface that might reflect an image of a card face.
>
> "*Calling and recording.* You are now ready to attempt to call the top card, thus testing your ESP of objects. Do this in the same easy, natural manner suggested for making a call in Test I. . .
>
> "When you have made your choice for the top card, write it on the record sheet, remove the card, and place it on the table without looking at the face. Now decide in the same way what the second card is, record your call, and remove the card, placing it face down upon the first one. Do the same for the third card, the fourth, and so on until all twenty-five have been called.
>
> "As in Test I, you may not feel sure of your calls as you make them. Go right ahead, doing your best and waiting until the results are checked to learn of your success.
>
> "*Checking up.* When the run is completed, turn the cards over and record the actual order beside the calls. Mark the hits clearly, and put the score at the foot of the column" (pp. 15–16).

Experiments using these methods in which positive results for the clairvoyance hypothesis have been reported are those of (1) Rhine (**83**), with the BT method checked up after 5 calls and after 25 calls; (2) Woodruff and George (**128**), in which the OM and BM tests were introduced and the BT method was also used; (3) Pegram (**75**), with BT and OM tests; (4) L. E. Rhine (**90**), with OM; (5) Gibson (**33**), with OM, BM, and BT methods; (6) Humphrey and Clark (**48**), who used the BT method; (7) "Anonymous Scientist" (**2**), with the BT method; (8) Pratt and Price (**79**), using the BM method; and Warner and Raible (**122**), with OM and BM. Evidence that some of the cards actually do afford visual cues under certain conditions may be cited.

Three kinds of cards have been used in the ESP experiments. In the early work, reported in the "Monograph," experiments were performed with Zener cards and an earlier version of the present ESP cards. In "Letters and Notes" (**64**), the cards are described as follows:

> "In the beginning of ESP research at Duke University, when it was assumed as a working principle that the subject should, if he wished, be able to locate directly the card he was calling, the cards used were cut

(mostly die-cut) from heavy cardboard, were opaque to a 100 watt light, were hand-stamped with ink that left no tactual impression or warping, and were carefully inspected for secondary cues on the backs" (p. 72).

These were the Zener cards. The earlier version of the ESP cards, used in the majority of the "Monograph" experiments, were stamped with a rubber stamp on blank playing-card stock which had a filigree design on the back, as shown in the photograph on p. 48 in *New frontiers of the mind.* The commercial ESP cards are printed with heavy symbols on the faces and a design of white lines on the backs, which are otherwise solid blue.

Soon after the commercial cards were made publicly available, it was discovered that under certain conditions of lighting the symbols could be read from the backs of the cards. Gulliksen (**40**) made the further observation that these cues might be used unconsciously by a subject who was allowed to look at the backs of the commercial cards while matching them in the OM and BM methods and calling them in the BT methods. Kennedy (**59**) presented a case in which significant extra-chance scores were made by a subject who was allowed to look at the backs of the cards in the OM test. This subject reported that he was unaware of using cues on the backs of the cards. In the same paper, a photograph of the cues taken under optimal lighting conditions is also presented. Small marks put on the cards by shuffling and handling may also serve to raise the guessing level significantly above that expected by chance. A subject in the Stanford laboratory made high scores with the BT method and the earlier version of the ESP cards when the same deck was used over a long series and vision of the backs was permitted. She was unable to verbalize concerning the nature of the cues but examination of the cards showed small marks which might have been associated with the correct symbol unconsciously during the checking-up process. Because of the possibility of using small visual cues in the methods described above, the conclusion may be made that extra-chance results obtained by these methods do not satisfy scientific requirements for evidence in favor of ESP.

(b) *Auditory Cues.* The question of involuntary auditory cues arises in connection with several experiments reported in the modern ESP literature. Warner and Raible (**121**), in threshold judgments of weight discrimination, obtained extra-chance results when the experimenter knew the correct choice and chance results when the position of the correct choice was unknown. The writers state:

> "Critical consideration of the conditions leads us to believe all modalities beyond suspicion except hearing. . . True, it seems far fetched to

suppose that such cues could in any way be construed so as to aid in the making of correct judgments. It is also true that our subjects (and there seems no reason to doubt their word) were entirely unaware of any assistance from such sources or from any sources other than the feeling of the weights themselves. . . For this reason, further discrimination tests are being made. . . The experimenter and the subject, instead of being separated by a mere screen, are confined in non-communicating rooms" (p. 50).

Until further crucial tests are made, as suggested in the quotation, these results are made equivocal as evidence for the telepathic hypothesis by the possibility of minute sound cues. Bender (7), in a report on the alleged telepathic ability of a feebleminded girl, concluded that involuntary auditory cues were used by the subject in performing her unusual feats. Drake (26) also has worked with a feebleminded case which presents striking similarities to the one reported by Bender. Unfortunately, the Drake investigation, as reported, was not carried to the crucial stage of elimination of all possibilities for the use of cues. The experimenter writes:

"We hope it will be possible to continue the investigation using only rigid control conditions" (p. 195).

As suggested in the Warner-Raible quotation, rigid control conditions for auditory cues would involve placing sender and receiver in different sound-shielded rooms.

2. *"Mental Habits" and Preferences.*

The Pure Telepathy (PT) method in ESP research is described as follows in the "Handbook":

"*Getting ready.* This is a test for the ESP of a thought alone. No cards are used for the sender to look at. He simply chooses a random order of the ESP symbols and holds these in mind (without writing them down) until the receiver has made his choices. Success in this test depends entirely upon reading the sender's thoughts.

"When everything is ready for the test to begin, the sender decides what the first five symbols will be. He imagines an order of five ESP cards as they might come in a shuffled deck. Then he concentrates upon the first symbol, and signals to the receiver that he is ready.

"*Calling and recording.* The receiver sits where he cannot see the sender. When he gets the first signal he makes and records his call and signals back to the sender. The signals may be given by tapping with a pencil, as before. After the receiver has signaled that he has made his call, the sender writes down what symbol he had in mind.

"The sender then concentrates on the second symbol of the five, which has already been chosen, and gives the signal. The receiver records his call for this trial and signals back. When the first group of five symbols has been used, the sender chooses another order of five and concentrates on the first one of these. This goes on until the run of twenty-five trials is completed" (p. 50).

Mental habits and preferences may enter into the agent's selection of symbols on which to concentrate during this test. Kellogg (**56**) has criticized the PT method as follows:

> "Again, if experimenter and subject happen to have thought preferences in favor of the same one or two of the designs, scores will be increased regardless of any mutual influence. Any experimenter in such work will tend to form more or less definite order habits. This possibility was recognized [by Rhine], and precautions taken to counteract it. Cross-checks, applied to *other* series, show these precautions were on the whole successful. At any rate, habits did not become so established that similar plans were used in *immediate* succession. But many of the different arrangements of the designs, that would occur in the shuffling of the pack, would almost certainly not be included among the plans chosen consciously. Some might seem too systematic, e.g. aaaaa bbbbb, etc.; aaaaa bcd bcd, etc., . . . eeeee. On the other hand, others with no apparent plan would give no ready means of keeping to the equal frequencies desired. If such helter-skelter arrangements are avoided, the range of possibilities is reduced; if not, there is more scope for symbol preferences which may be correlated with those of the subject. Rhine, as a means of controlling frequencies, coached his experimenters to plan by groups of five, which leads most naturally to five sets of one each of the five designs, thus very much reducing the number of different arrangements. (From 623 trillions down to less than 25 billions, subject to further reduction by avoidances, etc.) This extreme he seems to have tried to prevent, but the measures taken for the purpose are not such as to result in anything like the total possibilities of shuffling. There has been no mention of any attempt to keep secret the instructions given for the conduct of the tests. It would be strange if students interested in the work did not sometimes discuss various possible arrangements of the symbols. However that may be, subjects would surely tend to follow somewhat the same general sort of procedure as the experimenters—which means a marked increase in the chances of high scores. As the subjects were informed of their success at the ends of the runs, adoption of plans of the type used by the particular experimenter would be favored. Skill could thus be developed just as it is in various familiar card games. Indeed, a good player might adjust to this experimental situation almost immediately" (p. 340).

Rhine's method (**84**) for determining whether or not preferences in choosing symbols helped to produce extra-chance scores was as follows:

> "The question of similarity in habits of choice may be further checked by cross-checking the agent's record with the percipient's of—let us say—the day preceding or following, that is, correlating runs not intended to

match. A cross-check was made of the Junaluska Series given below and it yielded an average of only 4 hits per 25. Habitual similarity of order of choices between agent and percipient can, I think, be said to have been satisfactorily eliminated. Moreover, in the series cross-checked as just described, the runs were made daily and the cross-check, therefore, covers also the question of daily routines or patterns of order of choosing.

"Finally, as an extreme test of 'patterning' or order habits, we may check the percipient's records against themselves in consecutive order. The percipient might have a pattern of order without its being coincident with a similar one supposed for the agent. But if *he* does *not* have such a pattern, the question of similar order-habits is ruled out. Such a check made on 12 of the percipient's records given below, taken consecutively from a block chosen at random, and on the 8 records of the Junaluska records, yields an average of coincidences of 4.6 between runs, as against an expected 5" (pp. 223–224).

As Kellogg points out, preference effects may involve such simple errors as the tendency to choose a different symbol for the following choice. Since apparently not all of the data have been checked for these factors and since there is some question as to the adequacy of the methods used to prove the absence of preferences and order habits, the status of extra-chance results obtained by this method as scientific evidence for telepathy is also open to question.

Perhaps the best large-scale example of the working of preferences to produce unacceptable results with respect to the telepathic hypothesis may be found in Goodfellow's report (**34**) of the recent Zenith Foundation tests for mass telepathy.[6] Fernberger (**32**) has also indicated the presence of preference effects in the Zenith experimental methods.

Willoughby (**125**), Wolfle (**127**), and Gulliksen (**40**) have discussed another source of error due to possible "rational inference" in the BT method when the subject is told his success or failure after each guess or after each 5 guesses. Wolfle states:

"If after each call or after each five calls the subjects are allowed to see the cards which they have called, they may infer rationally which suits are left in the deck in the greatest frequency and so increase their average score above five. Willoughby and Wolfle have pointed out this source of error and have obtained average scores of 7–9 with the use of rational inference but without (consciously at least) using any extra-sensory powers. If Rhine also used these methods some of his above-chance scoring is easily accounted for" (p. 949).

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[6] For results of previous broadcasting tests for mass telepathy, see Woolley (**129**) and Bird (**9**). Goodfellow's work has not yet been published in detail. Preference factors arose in the Zenith results because 2 alternatives and but 5 guesses per session were used. Under these conditions, the guessers had only 32 possible patterns (such as 1,1,2,1,2 or 2,1,2,1,2) from which to choose. Although all 32 patterns were equally likely to be chosen by chance as correct, the frequency rankings showed that the audience preferred some patterns over others, thus piling up more large and small deviations from chance expectancy than would be predicted by theoretical probability statistics.

The BT_1 and BT_5 methods have not appeared in published reports since the publication of the "Monograph."

3. *Errors in Recording.*

The task of the reviewer in the present section will be to establish the essential validity of 2 propositions: (1) The methods of recording the original data in the majority of ESP experiments do not eliminate the possibility of unnoticed errors in recording, and (2) the experimental conditions under which the ESP to be discussed in this section appears at its height are also those in which unnoticed errors in recording may be expected to occur. In the final analysis, of course, the validity of the first proposition rests on repetitions of the Rhine experiments with the possibility of checking on the accuracy of recording. Evaluation of the experimental conditions which favor the appearance of extra-chance scores will depend upon the findings of experimental psychologists on the determiners of attention and suggestibility.

(a) *Clairvoyance Methods.* Descriptions of the recording procedures utilized in the Open Matching, Blind Matching, and Single Card Calling methods may be found above in the discussion of the possibilities of the use of sensory cues. It should be noted that the recorder, or the person who decides how many hits were scored, knows in the matching methods which cards should have been matched to the key cards to yield high scores. In the Single Card Calling method the subject's guesses are recorded first, then the actual symbol is recorded in a small space directly opposite the called symbol. Sometimes, the subject calls aloud the correct order of the symbols to the recorder, sometimes the recorder leafs through the pack himself to make the check-up. "Hits" are circled after the records have been made, not at the time the correspondence is called. In many cases the number of hits is recorded and no record is made of the cards matched to the different symbols. A partial check on the accuracy of recording would involve counting the symbols to see if the necessary 5,5,5,5,5 frequency was present. When all the symbols are not recorded, it is impossible to apply this check.

The Screened Touch Matching method (STM), in which every possibility of sensory cues seems to be eliminated, appears to be open to the criticism of unnoticed errors in recording. The method is described in the "Handbook" as follows:

"This is a screened matching test in which you show where you want the cards placed by touching the key cards one after another while your co-worker holds the cards out of sight and places them into five piles as the choices are indicated. In the picture facing p. 34, the person on the right is taking Test VI.

"*Screen.* The screen is an upright board twenty-four inches square. At the bottom of the screen is an opening two inches high and twelve inches long. The key cards are to be placed in a row in this opening. Three inches back of the opening, on the opposite side of the screen from you who are being tested, is an upright board a little longer and higher than the opening in the screen. While this backboard keeps you from being able to see the cards in the hands of your co-worker, it permits him to see the key cards as you touch them.

"The test is made like any other matching test, except that you show by touching one of the key cards with a pointer where you want the top card of the pack placed and let your companion put it down for you.

"*Getting ready.* Arrange the key cards in a row before the opening in the screen on the other side from the low backboard. Use the eraser end of a pencil or some other pointer to touch the cards.

"*Matching.* The co-worker sits on the other side of the screen, with the backboard between him and the key cards. He shuffles and cuts the pack out of your sight, and gives the signal when he is ready. He watches your pointer through the opening in the screen, and as soon as it comes to rest on one of the key cards he takes the top card from the pack without looking at it and places it face down behind the backboard opposite the key card touched. Go right on touching key cards at your own speed. The cards can usually be put down fast enough to keep up with you.

"*Checking up.* When the run is completed, the person handling the cards counts the number of hits (cards correctly matched). The score is put down on the record sheet and the test continued through the usual four runs, or as many more as seem advisable" (pp. 27–28).

No check on recording errors appears to be possible with this method. The method would be improved if independent records of the matched cards were made by a person who does not know the key-card symbols at the time the recording is done.

Referring back to the description of recording methods in the Single Card Calling or Before Touching test, it is evident that the recorder has the record of the subject's calls before him while checking-up the card series. Unnoticed errors in recording may arise due to split attention and expectancy of extra-chance deviations. The recorder, instead of writing the symbol he sees in the deck, may unconsciously record the symbol which he sees in the call series, thus automatically producing a hit which will be accepted when he goes back over the record to mark the correspondences between card and call series. As a partial control on these errors, records should be rechecked for variations from the 5,5,5,5,5 pattern of cards as suggested by Carpenter and Phalen (**19**). This check is not completely satisfactory, since errors in transposing the position of symbols may be made. Apparently no check on accuracy of recording is available for the transposition error. The practice at the Stanford laboratory has been to use a different deck for each set of 25 guesses, the order of symbols in which has been previously recorded without the subject's knowledge before the experimental period. When the " correct " order is applied to the set of symbols recorded at the time of experiment, the presence or absence of errors in recording may be determined and permanently recorded.

The Down Through or Pack Calling test (DT) is described in the " Handbook " as follows:

" *Getting ready.* Place a well-shuffled and cut pack of ESP cards on the table. Get ready to record your calls for the cards just as in Test II. Follow the directions for making your calls as in Test I.

" *Calling and recording.* When you are ready, simply proceed to make your calls, but without laying the cards off the pack as you call them. Try to get an impression of what the top card is and record this. Then try to get the second card down in the pack, then the third, and so on through the entire twenty-five. The test is to see how many more hits you can get reading straight down through the pack than would be expected by chance. In Test II each card was laid off the pack after it was called. In the present case, however, none of the cards are removed until all twenty-five have been called.

" It is not necessary to keep the position of each card in mind as you make each call. Say to yourself before starting the run that you are going to read the cards right down through the pack; then take the impressions of the symbols in the order that they come most easily and vividly to you. If you are writing your own calls, you will know that you should stop when you have put symbols in all of the spaces for calls. When an assistant records for you, he will tell you when to stop. It is not necessary for you to count your calls as you make them in order to succeed.

" *Checking up.* Check your results as usual by recording the actual order of the cards alongside your calls. Be careful to take the cards in the order in which you tried to name them, beginning at the top of the pack.

> "*Going on with the test.* Shuffle and cut the pack and make the second run . . ." (pp. 21–22).

The same criticism of possible unnoticed errors in recording the card series applies to this test. As in the case of the Single Card Calling method, the check-up is accomplished either vocally or visually and the conditions of split attention still may be present.

The distance clairvoyance tests also appear to be liable to the criticism of unnoticed errors in recording. Rhine (**85**) states:

> "In 26,125 trials conducted during the summer of 1937 by Mr. James MacFarland of Tarkio College, the comparison of score averages in relation to various distances indicates again that distance is not a limiting condition.
>
> "The tests were made with the DT technique, the cards being kept intact in packs by the experimenter throughout the test period and removed only when checking up. The subjects filled out five columns of a record sheet, one for each pack in the experimenter's desk, at any time they wished on a given day, and sent the sheet to the experimenter to be checked. Double checking was carried out, and the general sponsorship of Dr. R. W. George, Head of the Department of Psychology, was exercised over this series. The cards were well shuffled and kept under careful observation by the experimenter" (p. 180).

In the Duke-San Diego series reported in the same article, specific descriptions of recording methods are not given. It may be assumed that the check-up was made with the subject's calls available to the recorder as in the usual DT experiments.

Recording in the "Campus Distance Series in Clairvoyance" is described by Rhine (**86**) as follows:

> "The observer and subject synchronized their watches, and arranged to work at a stated time and distance. At the specified time the observer would take the top card from a shuffled pack of ESP cards in the room agreed on and lay it face down on a book in the center of the table without looking at its face. Thirty seconds later the subject in his cubicle in the Duke Library would record a call for the card. At the end of the minute, the observer would remove the card and take the next one. The cards as removed would be kept in order for later recording. Two runs were made per day. . .
>
> "In Groups A–D, the records were sealed up after each sitting and delivered to me before subject and observer got together" (p. 287).

The acceptability of this series of extra-chance results with respect to recording errors depends upon when the check-up was made and whether or not it was done with knowledge of the subject's

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calls. Apparently the general methods for clairvoyance research are open to question on the basis of recording errors.

Specific accounts of method in experimental articles on clairvoyance in the ESP literature may also be reviewed with respect to control of possible recording errors.

Rhine (**83**) used the BT, DT, and distance BT and DT tests in obtaining the "Monograph" results. Control of these methods has been described above. Pratt (**78**) was able to obtain extra-chance results with the Blind Screen Touch Matching method, in which the key cards were placed in boxes and their order was unknown both to the subject and to the experimenter until the check-up for hits. The check-up is described as follows:

> "When the deck was sorted out, the screen was removed, the experimenter turned up the key cards and recorded their order, and the actual distribution of the failures and successes under each key symbol was recorded and checked by both the subject and the experimenter" (p. 15).

Woodruff and George (**128**) used the OM, SOM, BM, SBM, BT, and SBT, the latter being the screened variety of the former, with success in guessing above the chance level reported. Objections to the check-up by these methods are to be found above.

Carpenter and Phalen (**19**), with the BT and DT methods, made the following observation concerning the check-up for hits:

> "The two experimenters were used in Series 7 after G [the best subject] had made an error in reading off the true distribution. The error was apparently due to his divided attention during the checking process. He would look both at the card and at the record sheet and in so doing called once or twice when observing the symbol on the record sheet rather than the card's symbol. This error was discovered early in the series and checks of records indicated that it possibly had not previously occurred" (p. 39).

Martin (**67**) added a variation to the DT method in that the subject recorded his own correct order of symbols. The method is described as follows:

> "The experimenter shuffled the cards thoroughly, cut them and placed them on the table before her. Exceeding care was taken that the bottom card was visible to no one. The subject then recorded twenty-five guesses on a record blank. The experimenter then read the actual order of the cards to the subject who recorded them. The experimenter carefully watched the recording of each card and was often checked by a third person. . . It should be noted that neither the subject nor the experimenter had any knowledge of the actual order of the pack until after all twenty-five guesses were made" (p. 186).

It should be noted that the subject had the order of calls before her while the experimenter was calling out the order of correct cards for recording.

Pegram (**75**), with the OM, BT, and DT methods, found that the direction of extra-chance scores could be controlled voluntarily by the subject. But the direction of unnoticed errors in recording may also be involuntarily controlled by the aim of the recorder, as may be shown experimentally.

Martin and Stribic (**68**) report the use of the DT test with the following method:

> "The DT or 'down through' method was used exclusively. The experimenter shuffled and cut the pack, and placed it face down before her, taking care to avoid seeing the bottom card. The subject then recorded his guesses for the order of the pack. After the completion of each run, and in the presence of both subject and experimenter, the pack was checked with the guessed order" (p. 24).

In a report by Humphrey and Clark (**48**), the BT test was used with no description of recording method given. In the case of Pratt and Price's (**79**) use of the BM test, recording was carried out as follows:

> "At the end of each run the score was obtained by the experimenters, who turned the key cards out face up upon the table and then sorted the 'hits' in each pile from the 'misses.' In addition to counting the hits as they were sorted, both experimenters recounted and the score was recorded immediately" (p. 88).

No check on the accuracy of recording is possible with this method.

Shulman (**97**) worked with psychotic subjects with the Screened Touch Matching test. His recording technique is given as follows:

> "After all of the 25 cards had been guessed, they would be checked to see how many had been matched with the correct key card. . . The cards were checked after each run of 25 cards and the subject was told his score" (p. 98).

Sharp and Clark (**96**) reported extra-chance scores with the OM, BM, DT, STM, and General STM methods. The last method, a variation of STM, is described in the following way:

> "In GSTM the procedure was to have one experimenter look at the cards (held out of sight under the inclined screen). The experimenter holding the cards did not know the location of the key cards, which were placed by a second experimenter and the subject" (p. 127).

The necessary controls for recording errors are not mentioned in this article.

Stuart (**109**) has found recently that subjects performed at an extra-chance level in the matching tests when they were allowed to match at their normal tapping rate. Matchings done at a rate other than the normal rate yielded chance results. In discussing the possibility of error commission in recording as a cause of the observed deviation Stuart states:

> " The method of recording in the screened matching procedure was as follows: After the subject had finished the run there were, on the table before the experimenter, five piles of cards, each in front of the symbol the cards were intended by the subject to match. Each of these piles was then turned face up and separated into two groups, the cards which matched the key card in front of the pile, and those which did not. As the correct matchings were thus sorted out they were counted. When the sorting was finished the correct matchings lay face up directly adjacent to the key symbols, and the first counting could be checked at a glance" (p. 179).

Apparently complete records were not made, nor was the recording done independently.

An adequate sample of recording methods in articles in which unsatisfactory clairvoyance has been obtained has been presented. The general rule is suggested that recording be carried out independently in order that the criticism offered may be eliminated.

(b) *Telepathy Methods.* Two methods for studying telepathy have been used by Rhine and others who have repeated his experiments. Unfortunately, the recording methods given in the " Handbook " for these 2 tests do not correspond to methods actually used in experiments in which high positive deviations from chance expectancy were obtained. The Pure Telepathy test was described in the section on mental habits. The reader may see from the description given there that the criticism of nonindependent recording applies since the agent usually acts both as sender and recorder of the percipient's vocal calls. In " Some selected experiments in extra-sensory perception " (**84**), Rhine appends a footnote:

> " The question has been raised as to whether the agent might not, through her strong interest in getting good results, be likely to mistake unclear enunciation by the percipient who is off in another room, and to give favorable interpretations of calls not clear. First, the names of the symbols are phonetically quite easily distinguishable; each has a different vowel sound. Second, audition was good with open doors between rooms. Third, the scores for this work with the agent and percipient in the same room are nearly as high as with the two separated " (p. 224).

Independent records could have been made by placing a recorder in the room with the percipient and having the agent record only the cards, the number of hits to be determined by juxtaposing the 2 records and noting the correspondences. As stated above, the "Junaluska" Pure Telepathy series, although apparently controlled for independence in recording, is not satisfactory from the "mental habit" aspect.

Kellogg (**56**) has also discussed the auditory illusion criticism of the Pure Telepathy test:

> "The experimenter had to try to think of the various designs as nearly as possible five times each in a run, but with no written plan to follow, give buzzer signals to call the attention of the subject for each trial, record the responses as heard, or supposed to be heard, check them if correct. Judge the effect on the scores of the almost inevitable tendency, especially in a laboratory so full of faith in ESP, to give the subject the benefit of the doubt in any trial involving some difficulty in hearing or in memory, and the great likelihood of illusions in hearing, the hearing of one call as a different one, especially when the observer gets excited in the course of a good run, and so hears what is desired instead of what is really uttered, yet with not the slightest realization of the possibility of a mistake. The much greater ease in following a lecture or play in one's own language has often been explained as due not so much to difference in understanding what is actually heard, as to the readiness to fill in gaps in auditory stimulation and, as far as conscious experience goes, hear the whole. In such a case, expectation is guided by the context, in line with the familiar usages of the native tongue. In the telepathy tests, success may breed success, in the record, by creating such an attitude of optimism that the observer-experimenter will strongly tend to hear the name of the symbol he has just been 'sending'" (p. 340).

Coover's work (**22**) on sound assimilation also shows that:

> ". . . the ear cannot be trusted to report correctly names or phrases, when the latter are spoken under such conditions as are deemed by the recipient satisfactory for communication yet which permit some degree of indistinctness . . ." (p. 407).

The second telepathy test, devised by Rhine, is the Telepathy Card or Undifferentiated ESP method. In the "Handbook" a description of the test is given as follows:

> "Two people work together in this test. One person shuffles and cuts the pack of ESP cards and looks at the face of each card while you, who are taking the test, try to name it. For convenience, we will call the person looking at the card the 'sender' and you the 'receiver.'
>
> "*Getting ready*. Fill in the blanks at the top of one of the record sheets to be found in the ESP Record Pad. It is best to leave these sheets in the pad as part of your permanent record.

"The sender shuffles thoroughly and cuts a pack of ESP cards. He holds the pack with the faces toward him so that he sees the bottom card. He concentrates on the symbol on the face and signals to you by tapping with a pencil when he is ready.

"Sit where you cannot see the faces of the cards. You may close your eyes or look off into space, or even look at the backs of the cards. You may relax bodily or sit at alert attention.

"*Calling and recording.* When you hear the signal try to get an impression of the card at which the sender is looking. Do this in your own most natural way. You simply want to give the correct name of the card. You cannot reason out or force the correct answer. Simply decide upon the one which comes to you most easily and vividly.

"When you have decided, write your choice for the top card into the first space for 'calls' on the record sheet. In recording, let O stand for circle, + for plus, ∟ for square, ∼ for waves, and ∧ for star. As soon as you have finished, tap with your pencil to signal the sender that you are ready for the next. Do not tell him what your call was.

"The sender then removes the card at which he has been looking and places it face down on the table without saying what it was. He looks at the second card and signals again when he is ready. Make your call for the second card and signal that you are ready for the third. Continue in this way until the run of twenty-five cards is completed.

"Choose your own speed. From one to five minutes to the pack is a favorable speed after you have become familiar with the test.

"You may not have the feeling that you are right in your calls as you make them. Many do not. This feeling does not always go with success.

"You must not know your success or failure on any card during the time that the run is in progress, since this would permit you to keep track of the cards. It is better if there is no talking during the run, but of course it can do no harm to talk between runs.

"*Checking up.* When you have finished calling all twenty-five cards, both of you check the score by turning the pack over and recording the order of the cards in the column marked 'cards.' You scored a hit every time the call and the card are the same. Mark these clearly, and write the total score for the pack at the foot of the column" (pp. 11–13).

According to this method, the percipient makes his own independent record of the calls. The check-up is carried out according to the questionable method of recording cards with knowledge of the calls. In a photograph opposite p. 19 of the "Handbook," with the title, "The Telepathy Card Test," the agent has the record book and appears to be recording the subject's calls. An earlier version of the Telepathy Card test, one that corresponds with the illustration in the "Handbook," may be found on the "Instructions" card in the new ESP decks. To quote:

"*Telepathy-Card Test.* For two persons. One acts as sender, the other as receiver . . .

"When both are ready, sender picks up pack holding face toward him, concentrates on first card and taps with his pencil on the table. Receiver tries to call the card, calling either the *first* one that comes to mind or that one of which he gets the clearest image, as he prefers. Allow only one trial for each card. The sender records the call in the 'call' column of the record sheet . . .

"The cards are kept in order until the end of the pack, then turned over and recorded in the *card* column as they are turned."

Possibilities for errors in recording the percipient's calls under these conditions are numerous and the direction of the errors is "channelled" to produce high scores automatically. When the agent is concentrating on the symbol in the deck, a reaction tendency to record that symbol is set up. The symbol called by the subject usually has the "right of way" to the final response of recording. Lapses of attention, "automatisms," expectancy of high scores, and suggestibility may give the symbol on which the agent is concentrating the "right of way" to the recording response and it may be recorded instead of the called symbol. When the actual card symbol is recorded later alongside of this error, a hit is automatically scored. Thus, the Telepathy Card test, in the form which seems to have been used in experiments reporting positive results, is uncontrolled for unnoticed errors in recording both in the card series and in the call series.

Experimental articles in which the latter version of the Telepathy Card test has been used are: (1) Rhine's "Monograph" (**83**) [pp. 59, 66] and (2) Gibson's article (**33**) in which no specific description of method was given. Kubis and Rouke (**62**), working with twins, obtained essentially chance results with a modification of the GESP technique which involved independent recording. Two selected batches of trials, however, were thought to indicate extra-chance results. Bond (**13**) has reported extra-chance scores with a group of retarded children. Specific details as to recording methods are not given.

(c) *Precognition Methods.* Rhine (**88**) has recently adapted the DT, OM, and the STM methods to the study of precognition or prophecy. His description of method is given as follows:

"Most of the 15 series of tests reported in this paper are based on the calling-before-shuffling modification of the DT procedure, called precognitive DT or PDT . . . The calls are made and recorded and the pack of cards then shuffled (face down) by the experimenter and checked against the record of the calls. The amount and type of shuffling varied somewhat with different investigators but consisted mainly of the dovetailing method, and of at least two such shuffles.

"In making the calls in these PDT tests the subject either wrote down the symbols or called them orally to a recorder. This record in earlier series was checked against the pack by the experimenter (with the subject as witness) after the 'random rearrangement' shuffling was done. In later series a record of the card order was made and the checking done by comparison of call and card records, thus permitting later re-checking.

"The exceptions to the test procedure referred to above consisted in adaptation of simple matching techniques to the precognition problem. One of these involved the open matching (OM) procedure. In this modification (POM) the cards were laid face down before a set of five blank spaces instead of the key cards, the key cards to be supplied by chance after the target deck was dealt out. The selection of the key cards afforded the 'random rearrangement' since these were chosen by a specified routine procedure from a second pack which was shuffled by the experimenter after the target deck was dealt. The subject of course tried to match the cards dealt against the set of key cards that were yet to come.

"In a similar way the screened touch matching (STM) method was adapted to the precognition research (PSTM). The key cards were chosen as just described, and the subject indicated his calls or choices by pointing to one of five empty shallow boxes which *would be expected to have placed in it later* the card to match that which the experimenter had on top of the pack held behind the screen.

"In all but one series the experimenter did the checking with the subject also witnessing when this was not prevented by distance. But in the last two series, two witnesses were introduced during the shuffling, card recording, and the checking of correspondences in the call and card records" (p. 47).

Apparently, in all of these methods the recording was not done independently. Rhine states that a record of the card series was made but does not say whether or not this record was made without knowledge of the subject's calls.

In the second paper in the precognition series by Rhine, Smith, and Woodruff (**89**), the writers state that, with deliberate intent, subjects can shuffle a pack of ESP cards and match symbols with a similar deck shuffled by an experimenter or a recorded order of cards unknown to the subject with significantly better than chance success. The procedure is as follows:

> "Following the shuffling of the shuffler's pack, the experimenter and subject (when the subject was an adult) laid off the cards of the pack in the order of their occurrence, observing matching or failure to match, and recording the results in numbers of hits (i.e., correct correspondences). When children were investigated, the experimenter himself or the experimenter and an observer—not a subject—handled both packs of cards. When a recorded series of symbols were used instead of the target pack, the record of the shuffler's pack was taken on the standard record sheet and coincidences with the target series checked in the usual manner.
>
> "The greater part of the more crucial subdivisions of the data of this report have been independently re-checked for errors. This applies to all of the work in which a sealed target pack was used, and to approximately half of the screened target pack subdivision. Two experimenters (not subjects) were present throughout the experimentation and checking of the data for a substantial portion of the tests which yielded the highest score averages" (p. 121).

Apparently, complete records were not made in some of the experiments and no check can be made on order inversions in recording the deck in the experiments in which a prepared order of cards was used as the "target." No amount of "independent" rechecking after the experiment will bring to light such errors. Checking for the 5,5,5,5,5 frequency of symbols in the card column would be of some aid. This is not specifically mentioned.

(d) *Evidence in Favor of the Unnoticed Error Hypothesis.* Two important papers have already appeared in the *Journal of Parapsychology* which seem to indicate the validity of the criticisms discussed here in attempting to find explanations for these ESP results. Gibson (33) compared various ESP methods as to efficacy in producing extra-chance results. He concluded:

> "If the averages of all of the eleven subjects in each technique are ranked from highest to lowest, they are as follows: OM, GESP, BM, DT, GESP (long distance), STM" (p. 269).

In Table II the possible sources of error are listed with the methods. Good agreement between possible sources of error and experimentally demonstrated potency for producing extra-chance results is found.

The second paper, written by Greenwood (36), describes methods used in an empirical control series, the aim of which was to demonstrate conclusively that the mean correspondence in matching shuffled ESP decks with 100 sets of calls is exactly 5. The calls were not made with the intent of matching any of the decks of cards. The following quotation seems to add further evidence for the error hypothesis:

"At the end of 7,000 runs a random sampling recheck of 200 runs netted two errors of omission, lowering the score by one in each case. It was therefore decided to recheck the whole 7,000 runs or 175,000 separate comparisons. Accordingly, the writer went down one group of results and one or two assistants from the Parapsychology laboratory checked on another part. The procedure was to obtain the new result first and then compare it with the old record. All told, 81 mistakes were discovered, 72 of omission by one and 9 of additions to the score by one.

TABLE II

THE RELATION BETWEEN POTENCY FOR PRODUCING "GOOD" SCORES AND SOURCES OF ERROR IN SEVERAL ESP METHODS

Method	Errors
1. Open Matching (OM)	Backs of cards seen by subject. Both types of minimal visual cues can be used. Nonindependent recording.
2. General ESP or Telepathy Card Test (GESP)	Possible attentional and illusory errors in recording calls. Possibility of checking-up errors.
3. Blind Matching (BM)	Backs of cards still available to subject. More difficult because key cards are face down. Nonindependent recording.
4. Down Through or Pack-Calling Test (DT)	Possibility of checking-up errors in recording cards.
* 5. General ESP (long distance) (GESP l.d.)	Possibility of checking-up errors in recording, although least likely in this method.
* 6. Screened Touch Matching (STM)	Nonindependent recording.

* The difference in scores obtained by the 2 methods is small and appears to be nonsignificant.

"From runs 7,001 to 20,000, the end of the series, the same general procedure was used with the exception that the assistant and writer made *independent* records of scores, working on different parts of the series. At intervals the two records were then compared for differences. If differences were encountered these particular runs were carefully rechecked and errors corrected. . .

"For the whole series there were 12 errors of addition of one hit to a run, one of addition of two hits, and 77 of omission of one hit in the author's record. The total was 90 errors lowering the score by 63 hits out of an expected 100,000 hits. Since an urgent consideration of this series was the avoidance of errors on even the first going over, it would seem appropriate to stress the fact that with a little less rigor the errors would undoubtedly have been much more numerous. In particular, since omissions were the more common type of error, work which gives rise to a significant *negative* deviation should call for rechecking. This type of error would of course only serve to diminish a positive deviation and the misleading effect be at least a safe one" (pp. 140–141).

These precautions in recording and checking the data given above cannot be found in the great majority of experimental articles on ESP in which extra-chance results are obtained. MacFarland (**66**) has recently published a set of extra-chance results with the GESP and DT methods in which "the experimental set-up was designed to eliminate the possibility of sensory cues and recording errors." Sensory cues were eliminated by distance. Recording errors in the call column were eliminated by checking 2 independent records of the subject's calls. But the check-up of actual order of cards was conducted with the calls available to the recorder. Thus:

> "Both experimenters then checked the two decks of cards against the doubly-recorded order of calls. In this way there was secured double witnessing of all checking" (p. 162).

This is an unsatisfactory condition for elimination of errors in the card column.

It has been sufficiently demonstrated that the Rhine methods may allow either positive or negative errors to be made at the time of recording the original data. No satisfactory check to determine whether or not errors were made has been reported. Apparently, recording in these ESP experiments may be manipulated to prove a preconceived hypothesis of the recorder without his awareness of the manipulation. This is not without precedent in the literature of experimental psychology when records were ambiguous and the recorder worked with a strong motivation to produce results favorable to an hypothesis.

4. *The Laboratory Conditions for Producing the Majority of ESP Data.*

The second proposition to be established by reviewing the ESP literature is that the conditions under which extra-chance scores seem to be obtained are those which foster unnoticed errors in recording. These conditions in the recorder, as stated above, are expectancy of favorable results, split attention, excitement, and amenability to suggestion.

It is interesting to note that modern experimental psychology had its inception in the study of the "personal equation" of errors in observation due to divided attention. The articles of Münsterberg (**72**), Bauch (**6**), Crosland (**25**), and Kollarits (**61**), to mention but a few outstanding studies, have contributed to the knowledge of sources of error in human perception and the conditions under which errors are made.

In Rhine's (**83**) Appendix to Chapter 15 of the "Monograph," entitled "Suggestions to Those Who May Care to Repeat These Experiments," the conditions are given for the production of extra-chance scores with Rhine's experimental methods. These suggestions are specifically made to aid in the selection of subjects who may produce good results. To quote:

"It is hoped that others will repeat these experiments or, better still, perform more advanced ones. Much depends upon the conditions of the tests as to whether success or failure will follow. The following suggestions along with the discussion in Chapter 12, may help to avoid failures:

"1. The subject should have an active interest in the tests and be fairly free from strong bias or doubt. These would, of course, hinder effort and limit attention. An open-minded, experimental attitude is all that is required. Positive belief is naturally favorable but not necessary.

"2. The preliminary tests should be entered into very informally, without much serious discussion as to techniques, or explanations or precautions. The more ado over techniques, the more inhibition is likely; and the more there is of explanation, the more likely is introspection to interfere. Playful informality is most favorable.

"3. If possible to do so honestly, it is helpful to give encouragement for any little success but no extravagant praise is desirable, even over striking results. The point is that encouragement is helpful, apparently, but only if it does not lead to self-consciousness. If it does, it is quite ruinous. Many subjects begin well, become excited or self-conscious, and then do poorly. . .

"5. It is highly important to let the subject have his own way, without restraint, at first. Later, he can be persuaded to allow changes, after he has gained confidence and discovered his way to ESP functioning. Even then, it is better for him to have his way as far as experimental conditions can allow. It is a poor science that dictates conditions to Nature. It is a better one that follows up with its well-adapted controls and conditions.

"6. It is wise not to express doubts or regrets. Discouragement seems to damage the delicate function of ESP. Here again no doubt personalities differ. One subject, I know, has worked in the face of doubt expressed; but she is exceptional in this . . .

> "12. It is best to try good friends for Pure Telepathy at first—or couples, single or married, who feel certain they have thought-transference; and, above all, to try those people who say they have had 'psychic' experiences or whose ancestors conspicuously have had.
>
> "These are suggestions, not rules, for we do not yet know enough of the subject to lay down rules. They will help toward success, without endangering conclusions. One can always tighten up on conditions before drawing conclusions later. But any investigator must first of all get his phenomena to occur—or exhaust the reasonable possibilities in trying to" (pp. 166–168).

Attitudes of expectancy of good scores, "playful informality," and positive suggestibility in the subject seem best to help the unnoticed or unconscious use of sensory cues. But it is these conditions at work in the experimenter or recorder which seem to be most important in attempting to understand the production of ESP. It should be noted further that the encouragement of "playful informality" by the experimenter involves the condition of split attention which is a favorable if not an absolutely necessary condition for unconscious error production.

Excitement during experiments appears to be common. A sample may be taken from Rhine's (87) description of conditions in which a subject made 21 hits out of 25 calls:

> "For some time we drove along quietly. Then it occurred to me to test my subject on the way to the place where I had planned to make our first stop. I pulled the car up at the side of the road but did not bother to turn off the engine. Putting a large notebook across Linzmayer's knees, I took a pack of ESP cards out of my pocket and held it in my hand. He, meantime, had leaned back with his head resting against the top of the seat, so that his eyes saw nothing but the roof of the car. There were no mirrors or shiny surfaces into which he could have looked for possible reflections. During the actual progress of the test, his eyes were closed.
>
> "After giving the pack a cut—neither of us knew the order of the cards in it anyway—I drew off the top one and tipped it toward me just enough to catch a glimpse of the symbol and then put it face down on the notebook on Linzmayer's lap. Without looking at it or touching it he said, after a pause of about two seconds:
> 'Circle.'
> 'Right,' I told him, drew off the next card, and laid it on the notebook.
> 'Plus,' he said.
> 'Right.'
> 'Waves.'
> 'Right.'
> 'Waves.'
> 'Right.'

> At this point I shuffled the deck again, cut it once more, and again drew off a card.
>
> 'Star,' Linzmayer said when the card was placed on the notebook. It was a star.
>
> When he had called *fifteen cards in succession* without a single mistake, both of us were too amazed for a while to go on with the rest of the run. No conceivable deviation from probability, no 'streak of luck' which either of us had ever heard of could parallel such a sequence of unbroken hits. We both knew that the thing Linzmayer had just done was virtually impossible by all the rules in the book of chance, but he had done it . . .
>
> "No reader of this book need consider the account of this extraordinary run of Linzmayer's as presumptive evidence that ESP is a fact. The conditions of the test were not our usual laboratory ones, and the scientific evidence for ESP rests upon work performed under the strictest conditions. Write that amazing score off, if you like, to mere exploration. With all of the scepticism I can muster, though, I still do not see how any sensory cue could have revealed to Linzmayer the symbols of those 21 cards he called correctly" (pp. 77–79).

Confirmation of the present interpretation of the meaning of the experimental conditions prescribed by Rhine for ESP research is found in an article by Pratt and Price (79) on the subject-experimenter relationship. Blind Matching and Pack Matching tests were used and independent records were not made. "Favorable" and "unfavorable" conditions for high scores in ESP were defined as follows:

> "The 'favorable' condition previously mentioned consisted of introducing a subject to the situation by one-half hour of general conversation before starting the tests and *then continuing the conversation during the test*.[7] The 'unfavorable' one was characterized by starting to test a subject without delay and in deliberately keeping him out of the conversation as much as possible" (p. 91).

The condition of divided attention in the experimenters may define ESP scoring ability in the subjects.

In closing this section on the conditions in subject and experimenter which foster ESP, some attention should be given to the general emphasis placed on "witnessing" by Rhine and those who have obtained evidence for ESP outside the Duke laboratory. It is an everyday observation that witnessing may, under some conditions, be absolutely untrustworthy. Witnesses, to serve any useful function, should take independent records of the calls and cards so that the accuracy of recording may be ascertained later.

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[7] Italics mine.

Münsterberg (72) has discussed the well-known errors in human "witnessing" as they appear in courts of law. The general conclusion has been that "witnessing" is unreliable since a suggestible witness or one who expects high results may pay attention only to nonessential factors in the situation.

TABLE III

THE "PSYCHOLOGICAL PHENOMENA" OF ESP

1. ESP usually exhibits an "insight" learning curve (**83**, p. 164).	Errors of attention and illusion are not products of gradual learning; hence their sudden intrusion as "insight."
2. Some agents are better than others (**83**, p. 101).	Some people are more prone to make these errors than others.
3. ESP fluctuates, waxes, and wanes (**83**, p. 137).	Errors in recording depend upon special conditions of expectancy, suggestibility, and inattention.
4. A carefree, playful attitude in both experimenter and subject is necessary for good ESP (**83**, p. 167).	These attitudes may also be best for unnoticed errors in recording.
5. Distances may increase extra-chance scoring (**85**).	When the percipient calls vocally in telepathy experiments, the chance for recording errors is increased; when the calls are later checked against the cards in clairvoyance experiments, errors may occur.
6. Drug results. High with caffeine, low with sodium amytal (**83**, p. 163).	Recorder knew what effect the drug ought to have. Caffeine may increase ability to use minimal cues; sodium amytal may decrease it.
7. Fatigue in the subject may decrease ESP scoring (**83**, p. 128).	Fatigue may lower subject's sensitivity to minimal cues.
8. ESP may be voluntarily controlled (**75**, pp. 204–205).	The direction of unnoticed errors may be controlled by recorder's preconceived hypothesis.
9. Certain psychoses may be differentiated by ESP tests (**97**, p. 104).	Same as 8.
10. Blind persons have ESP (**80**).	Same as 8.
11. The presence of sceptics disrupts good ESP scoring (**83**).	Sceptics may watch for sources of error and disrupt "playful" attitude.

From the above considerations of the "atmosphere" of the ESP laboratory, alternate explanations for the "psychological phenomena" of ESP research may be offered. Table III lists the phenomena and the present writer's conclusions as to their most probable explanation. It may well be that the absence of "playful informality" and consequent control of conditions in the experiments of Adams (**1**), Baker (**3**), Cox (**24**), Soal (**103**), and Willoughby (**126**) will help to explain the lack of confirmation of the ESP hypothesis in these cases.

B. "INEXPLICABLE" EXPERIMENTS

Thus far, the criticisms and suggestions for improvement of experimental control brought forward apply only to the type of ESP experiments discussed above, where the conditions of the experiment, as stated in the papers themselves, are open to question. In this section, 2 experiments will be reviewed more completely since the reported conditions were such as to eliminate the errors discussed above. Eventual explanation of these results appears to the present writer to rest on an entirely different basis than the foregoing ESP data.

Warner (**120**) reported a set of 250 trials in which the conditions of the experiment were given as follows:

> ". . . the experimenters, having told the subject that work was to begin, retired to the upstairsroom, shut the door, cut a shuffled deck of cards and placed the cut card face down on the table by itself. It was not seen by the experimenters until after the guess had been made and recorded by the subject in her room downstairs. The subject signalled when this had been done by pressing a button which flashed a light in the experimenter's room. When this signal was given, the card cut was exposed and recorded, and a different deck, newly shuffled, was cut to obtain the next card to be guessed. This procedure was repeated without interruption until the end of the work. At the conclusion, the subject's record of guesses was compared with the experimenter's record of cards cut" (pp. 236–237).

The average number of hits per 25 guesses was 9.30.

It is worth noting that recording was not completely independent in this experiment, since the signal could be varied in duration by the subject, thus providing a possible cue. Another unusual item about this report is the non-random distribution of frequencies of the card symbols: *i.e.* circle, 71; rectangle, 50; waves, 43; plus, 43; and star, 43. The chi-square, obtained by comparing theoretical and actual frequencies, is 11.76 with a P of .02 that repetition would give as bad or worse fit by chance. The call preferences, however, do not correspond with the most frequent card symbols. The possibility of variation in duration of the light signal serving as an unnoticed cue to the recorder should justify insistence on a repetition of the experiment under the same conditions with the card symbols recorded before the subject's guess and the uneven frequencies of the different symbols to be guessed eliminated. This technique involves the added possibility of telepathy but this should not affect the scoring if the experiment is repeatable. The experiment was done in the subject's home.

Very high scores have been reported by Riess (92) with a single subject who later lost the ability to guess at an extra-chance level and who is not available for further experimentation. This experiment is unusual for the high scoring rate and the strictness of the conditions in ruling out the errors of ESP. For example, on the 19th run all 25 guesses were correct. The hits per pack were, consecutively, 5, 7, 10, 12, 15, 8, 16, 13, 18, 21, 11, 15, 19, 24, 21, 21, 22, 24, 25, 24, 21, 20, 19, 18, 14, 15, 15, 16, 12, 19, 21, 22, 24, 20, 18, 22, 21, 19, 19, 18, 18, 19, 18, 17, 18, 19, 20, 20, 20, 19, 20, 21, and 21. A period of no work intervened, after which the scores were 2, 4, 7, 12, 7, 5, 4, 3, 5, and 4. Obviously, the scores in the first series eliminate chance variation as an important factor in the production of the results. The experimental method is given as follows:

> "The usual pack of ESP cards was employed. Since both the subject and the experimenter lived in the same suburb of New York City, the experiment was conducted in their respective homes. At 9:00 P.M. the experimenter would expose a card from the freshly shuffled deck lying on his desk. He looked at the card and noted the symbol on the mimeographed record sheet. At 9:01 P.M. the subject, seated at a table in her home, guessed at the symbol on the first card exposed by the experimenter in his home. The experimenter, meanwhile, had placed the first card to one side and at 9:01 had exposed the second card. This procedure was repeated until the deck was exhausted. It was then reshuffled manually and, after an interval of 10 minutes, the whole experiment was done again. In this way 50 trials were run off during each experimental session. The distance between the two homes was approximately one-quarter mile and the respective rooms in the houses were so situated that they faced in opposite directions to each other" (p. 261).

The recording was done independently, although apparently the subject was not witnessed. No information as to when and how the check-up for number of hits was made is included in the experimental report. Since the high scores reported in this experiment are so much at variance with the majority of ESP results, a full determination of the conditions under which the unusual results occur would seem to be of paramount importance.

In addition, the "Campus Distance Series in Clairvoyance," or the Pierce-Pratt series, should receive further mention, although this set of trials has been discussed already in the section on recording errors. If a fuller account of the method is given, demonstrating that the check-up was made with completely independent records of cards and calls, this experiment should be joined with those of Warner and Riess as being as yet inexplicable.

IV. STATISTICAL METHODS IN ESP RESEARCH

Two major problems have arisen from controversy over ESP statistical methods. The first problem has to do with the mathematics of theoretical chance expectancy; the second, with the effect of selection upon the statistical methods used. The first problem has been fully discussed by Willoughby (**123**, **124**, **125**), Kellogg (**55**, **56**, **57**), Rogosin (**93**), Heinlein and Heinlein (**46**), Herr (**47**), Gulliksen (**40**), Wolfle (**127**), Stuart (**106**, **107**, **108**), Greenwood and Stuart (**38**), Stuart and Greenwood (**110**), Huntington (**49**, **50**), Sterne (**104**), Lemmon (**63**), Greville (**39**), and Greenwood (**35**, **37**). Kellogg (**58**) states that essential agreement has been reached on this theoretical point. To quote:

> ". . . it is the mathematical aspect of the research upon which substantial agreement has now been attained. It is quite clear that the problems of selection and treatment of evidence as such, and of rigorous control of the experimental procedures, are approaching solution, but are not yet fully settled" (p. 148).

It is generally recognized, however, that the mathematics of chance expectancy does not indicate a functional relationship between variables. "Causes" for observed deviations from chance expectancy must be sought in the experimental conditions and controls, not in the mathematics of chance. This inductive "leap" from extra-chance scores to extra-sensory perception has been one of the underlying reasons for controversy.

Historically, one of the important considerations for evaluating guessing experiments in telepathy and clairvoyance has been the question of selection of data favorable to a preconceived hypothesis. No one will challenge the statement that extra-chance results can be obtained by selecting only deviations in the direction of the preconceived belief and putting them through a statistical mill which is not applicable to the scores because of the original selection. The selection-of-data error may be more subtle in its application in experimental situations. Three recent attempts to reproduce conditions thought to obtain in ESP experiments with respect to selection of data are available for analysis.

Warner (**119**) matched shuffled decks of ESP cards in order to obtain information about the "run of luck" explanation of extra-chance deviations. He selected 220 scores, from a large random distribution of hits per run, which were 8 or above. He then continued matching decks with respect to each of these scores until the total score (preliminary score plus further scores) fell below plus 1 sigma from the mean of 5 hits. Warner successfully demonstrated, then, that by chance alone, initial high scores tend to level out when enough random runs are added to the initial high score. Leuba (**65**), with the same general method, has demonstrated empirically that, as one would expect from theoretical considerations, significant deviations may occur on a purely chance basis.

Greenwood (**37**) recently suggested the use of a correction factor for the limit of chance expectancy, to be applied when the stopping point in a given series is a function of the results obtained up to that point.

V. CONCLUSIONS

In the present review, the hypothesis was presented that extra-chance results in the majority of ESP experiments may be explicable on the basis of unnoticed errors in methodology. The errors and methods in which these errors may possibly occur may be summarized as follows:

1. Selection of data. All the ESP methods are open to this criticism in one form or another.
2. Lack of independent record. This criticism also applies generally to the ESP methods, with the exception, of course, of the experiments described above as "inexplicable."
3. Sensory cues. Results obtained with the Open Matching, Blind Matching, and Before Touching methods may be explained on this basis.
4. "Mental" habits. The Pure Telepathy methods are open to this criticism.
5. Logical inference. The BT methods in which the subject is given knowledge of results during a single run (BT_1 and BT_5) are unsatisfactory for this reason.

Certainly, the sources of error discussed here should be eliminated from consideration by adequate experimental control. These controls are not complex nor do they involve any novel principles. Greenwood (**82**) has already presented the main points of these suggestions.

The controls suggested for the ESP methods follow:

1. Sufficient distance or shielding should be used to eliminate all possibility of sensory cues.

2. Records of calls and cards should be made independently by 2 different recorders. The check-up should be carried out by juxtaposing the 2 independent records.

3. A tested method for producing a random distribution of symbols in the card series should be used.

4. In order to eliminate the effect of "optional stopping," a limit of trials should be set before the experiment. It might even be advantageous to postpone the check-ups until after the experiment is terminated.

Concerning the "inexplicable" experiments, little can be said. Perhaps another rule to insure objectivity should be added to the foregoing, namely:

5. The experiments should be conducted under such conditions and auspices that the subject cannot be suspected of fraudulently producing the results.

This rule would probably involve independent testing of the same "good" subject in different laboratories.

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STUDIES OF MENTAL RESEMBLANCE BETWEEN HUSBANDS AND WIVES AND BETWEEN FRIENDS

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The question whether opposites or likes attract in friendship and marriage has held enough popular interest for opposing views to become crystallized in two contradictory proverbs. Attempts to answer it in quantitative terms with respect to traits of intellect and personality have in recent years received a strong impetus from the development of devices for measuring such traits. The present review will consider, first, the material on mental resemblances of husbands and wives, then the literature on friend resemblance, and will conclude with a comparative and critical summary.

STUDIES OF HUSBANDS AND WIVES

Reviews of the earlier literature on homogamy or assortative mating appear in studies by Jones (**23**), Schiller (**35**), and Schooley (**36**). This earlier material was quantitative in the field of physical characteristics, but largely subjective and speculative with regard to mental traits, particularly in the matter of personality resemblances. The field of temperament was the only one in which negative correlations were found.

The present article begins with the publications of 1928. In a number of cases the data concerning husbands and wives were incidental to a larger study of family resemblance, from which the material relevant to the present problem has been abstracted.

Table I summarizes the investigations of mental resemblance, classified, according to the type of characteristic studied, as intellectual, temperamental, or attitudinal. A compilation of results from a number of studies such as we have here leads one to look for agreements and disagreements. The similarity of the correlations in intellectual traits where the groups have been representative ones with a fairly wide range is worthy of note. Schiller's lower correlations may probably be ascribed to the homogeneity of her group of subjects. If the variability of the group has such a bearing on the significance of the correlation, one becomes conscious of the need for some indi-

TABLE I

SUMMARY OF STUDIES OF MENTAL RESEMBLANCE OF HUSBANDS AND WIVES

Characteristic	Investigator	N of Couples	r [1]	Remarks
Intellectual				
Intelligence	Willoughby (**46, 47**)	90		A representative group from the vicinity of Palo Alto, California.
5 Verbal Tests			.44 [2] (av. corr.)	
6 Nonverbal Tests			.44 (" ")	
Intelligence (Army Alpha)	Jones (**22**)	105	.598 [2] ±.042	Group was representative of the general Vermont population in socioeconomic status.
Intelligence (Stanford Rev.)	Burks (**5**) Group A	174	.42±.04	Groups chiefly from skilled labor, business, and professional classes.
	Group B	100	.55±.05	
Intelligence (Otis S-A)	Freeman, *et al.* (**12**)	150	.49±.04	Groups chiefly from skilled labor, business, and professional classes.
Intelligence (Otis S-A)	Schooley (**36**)	80	.556±.052	Range of school attendance for the men was 5 to 22 years.
Memory (List of 20 words)	Schooley (**36**)	80	.567±.051	
Arith. Reasoning	Schiller [3] (**35**)	46	.293±.090 (raw) .344±.088 (corr.)	Random pairs gave r of —.138±.097 (corr.).
Vocabulary	Schiller (**35**)	46	.109±.097 (raw) .117±.098 (corr.)	Random r (corr.) was .086±.198.
Information	Schiller (**35**)	46	.414±.082 (raw)	Random r (raw) was —.077±.099.
Associations (Kent-Rosanoff)	Schooley (**36**)	80	.468±.079	
Associations (Kent-Rosanoff)	Schiller (**35**)	46	(raw)	Raw r for random pairs:
Egocentric			.378±.087	—.390±.086
Supraordinate			.222±.097	.158±.099
Contrast			.468±.079	.041±.102
Inner			.041±.102	—.461±.080
Speech-Habit			.464±.080	—.121±.100

[1] Correlation coefficients are by the Pearson formula or its equivalent unless otherwise designated. It has not always been clear whether the coefficients were raw or corrected for attenuation. If correction is not mentioned, probably they may be assumed to be raw.

[2] Coefficients are calculated from sigma scores for each age group to eliminate influence of age.

[3] Schiller's subjects were a highly homogeneous group.

TABLE I—*Continued*

Characteristic	Investigator	N of Couples	r^1	Remarks
Temperamental				
Neurotic Tendency (Bernreuter)	Hoffeditz (**17**)	100	.160±.066	
Neurotic Tendency (Bernreuter)	Terman & Buttenwieser (**41**)	126	.108±.060	A group scoring high in marital happiness.
		215	.217±.044	A group scoring low in happiness.
Neurotic Tendency (Bernreuter)	Sward & Friedman (**39**)		(raw) (corr.)	
		56	.34±.08 .43	Jewish.
		56	.12±.09 .15	Gentile.
Neurotic Tendency (Bernreuter)	Crook (**9**)	79	.06±.070	
Neurotic Tendency (Thurstone)	Schooley (**36**)	80	.299±.069	
Neurotic Tendency (Thurstone)	Willoughby (**48**)	100?	.27±.05	Professional class.
Mental Hygiene Inventory (Woodworth-House)	Schiller (**35**)	46	.021±.098	Random *r* was .059±.098.
Extroversion-Introversion (Whitman's Shortened Scale)	Schiller (**35**)	46	.005±.098	Random *r* was .223±.093.
Introversion (Bernreuter)	Terman & Buttenwieser (**41**)	126	.020±.060	High happiness score.
		215	.165±.045	Low happiness score.
Self-Sufficiency (Bernreuter)	Hoffeditz (**17**)	100	.091±.067	
Self-Sufficiency (Bernreuter)	Terman & Buttenwieser (**41**)	126	.074±.060	High happiness score.
		215	.121±.045	Low happiness score.
Self-Sufficiency (Bernreuter)	Crook (**9**)	79	.00±.070	
Dominance (Bernreuter)	Hoffeditz (**17**)	100	.152±.066	
Dominance (Bernreuter)	Terman & Buttenwieser (**41**)	126	.241±.057	High happiness score.
		215	.289±.042	Low happiness score.
Dominance (Bernreuter)	Crook (**9**)	79	—.05±.070	
Dominance (Allport A-S)	Crook & Thomas (**10**)	66	—.07	
Attitudinal				
Attitude Toward 17 Ideals	Hunt (**19**)	62	Median correlation in rank assigned to these ideals:	
			.475	For married couples.
			.255	For 62 random pairs,

TABLE I—*Continued*

Characteristic	Investigator	N of Couples	r	Remarks
Attitudinal—Continued				
Opinions on Current Topics	Schiller (35)	46	.649	Shepard's U. U for random pairs was .476.
Attitude Toward War (Thurstone)	Newcomb & Svehla (29)	183	.434	Fathers and mothers.
		45	.528	Young husbands and wives.
Attitude Toward Communism (Thurstone)	Schooley (36)	80	.602±.048	
Attitude Toward Communism (Thurstone)	Newcomb & Svehla (29)	180	.578	Fathers and mothers.
		45	.712	Young husbands and wives.
Attitude Toward Birth Control (Thurstone)	Schooley (36)	80	.585±.050	
Attitude Toward the Church (Thurstone)	Newcomb & Svehla (29)	190	.757	Fathers and mothers.
		45	.672	Young husbands and wives.
Liberalism-Conservatism (Harper)	Morgan & Remmers (28)	17	.380±.149	
Religious Attitudes (Belief Pattern Scale)	Kirkpatrick & Stone (26)	78?	.56±.060	Rho
Values (Allport-Vernon)	Schooley (36)	80		
Theoretical			.370±.065	
Economic			.252±.071	
Aesthetic			.234±.071	
Political			.448±.060	
Religious			.380±.065	
			r	
			High Happiness Score N=126	Low Happiness Score N=215
Interest Constellations (Strong)	Terman & Buttenwieser (41)			
C.P.A.			.202±.058	.037±.046
Chemist			.125±.059	.076±.046
Lawyer			.424±.049	.167±.045
Life Insurance Salesman			.289±.055	.244±.043
Teacher			.242±.057	.295±.042
Y.M.C.A.			.358±.053	.133±.045
Office Clerk			.410±.050	.294±.042
Masculinity-Femininity			.029±.060	.085±.046
Interest Maturity (Strong)	Terman & Buttenwieser (41)		.300±.055	.256±.043

cation of the variability of the groups with respect to neurotic tendency, dominance, or attitude toward Communism, for example. The series of comparisons to which one is tempted by the array of correlation coefficients is of uncertain justifiability. The correlations for the Allport-Vernon Study of Values are particularly open to question, since the several value scores for a given individual purport to be relative, not absolute, measures of the weights which the several values carry for that individual, and having a high score in one value automatically lowers the individual's rating in the sum of the remaining values.

With all these qualifications in mind, we may make certain generalizations concerning the results. The correlations are higher in the intellectual and attitudinal traits than in traits of temperament. None of the correlations, even in temperament, are reliably negative, as the theory of attraction of opposites would require.

An extensive survey of the problem which is not included in Table I because it is still in progress has been reported in a preliminary way by Kelly (**24**). This study of 300 engaged couples, to be followed up annually for 7 years, includes measures on the Otis Self-Administering, Bernreuter, Bell, Strong, Allport-Vernon, and several attitude tests, as well as anthropometric measures, blood groupings, and life history data. A preliminary analysis for the first 100 couples shows correlations ranging from approximately 0 to .79, none of them being significantly negative.

Some of the investigations have considered the question whether resemblances in personality are more pronounced for couples that have been longer married. Hunt (**19**) found a correlation of —.05 between length of time married and similarity in ranking of 17 groups of ideals. Hoffeditz (**17**) compared 24 fathers and mothers aged 56 or more and 19 pairs of parents aged 45 or less with regard to resemblance in neurotic tendency, self-sufficiency, and dominance. She found no evidence that resemblance increases with duration of marriage. All the correlations were lower for the older couples, being slightly negative. The difference was least reliable for self-sufficiency, but approached reliability in the trait of neuroticism. Schooley (**36**), drawing the dividing line on the basis of length of marriage, and apparently at a considerably younger age in general, classified her 80 couples into a group of 40 who had been married from 1 to 4 years, and another group of 40 who had been married from 5 to 20 years. She found higher correlations for the longer-married group in neurotic tendency, in the free association test, in the economic and religious values, and in attitude toward birth control; lower correlations for the longer married in the theoretical and aesthetic values; and no difference with length of marriage in correlation of scores on political value or on attitude toward Communism. Her disagreement with Hoffeditz regarding neurotic tendency may be due to the different basis of division or to the small number of cases. Newcomb and Svehla (29) partly agree and partly disagree with Schooley's results in so far as their data permit comparison. The trend which they find in attitude toward the church is similar to that reported by Schooley with respect to the religious value, but in attitude toward Communism they report a lower correlation for "fathers and mothers" than for "young husbands and wives." In attitude toward war they find a lower correlation for the longer married. The reliability of these differences in correlation is not great, however, and the data do not permit conclusion as to whether the longer mutual association has caused a change in degree of resemblance or whether the traits under consideration have at different periods held different degrees of importance in determining mutual attraction before marriage.

The relation of personality resemblance to marital compatibility has been investigated by Terman and Buttenwieser (40, 41) and by Kirkpatrick (25). Terman and Buttenwieser compared the degree of resemblance between 126 happily married couples, 215 couples with a low "combined happiness score" on a marriage questionnaire, and 100 divorced couples (the latter are omitted from our Table I). Among the correlations on the Strong and Bernreuter scales, the only one which was significantly higher for the happily married than for the other 2 groups was in the Y. M. C. A. interest constellation. On certain individual items there was a distinct difference between the happily and unhappily married with regard to the amount of resemblance between the pair. Husband and wife were more likely to be happy if they were similar in attitude toward avoiding argument, and more likely to be unhappy if they resembled each other in admitted ability to "accept just criticism without being sore." Out of 130 items in which there was more than a chance difference between the relative amount of correlation yielded by the first group and the other 2, there were 96 in which agreement accompanied happiness, 30 in which agreement was associated with unhappiness, and 4 in which the relationship was uncertain. Terman and

Buttenweiser (**40**) also found that between groups selected for high and low happiness score there were consistent differences in scores for "common outside interests" and in agreement on 11 items which included "religious matters" and "philosophy of life."

Kirkpatrick (**25**) likewise found significant differences between unhappily and happily married couples in a measure of community of interest in a variety of activities. He considers these differences significant enough so that the "Relationship Family Interests" and "Individual Family Interests" scores might serve as an index for measuring marital adjustment and for predicting the success of a relationship.

STUDIES OF FRIENDS

Study of the resemblance between friends is an aspect of social psychology which has received increasing attention in recent years. Evidence of the recency of this interest appears in the temporal distribution of the 21 pertinent titles in the accompanying bibliography: 12 bear dates from 1931 to 1937 inclusive, 7 from 1922 to 1930, and the other 2 were published in 1898 and 1902, respectively. These studies reflect to a considerable degree the techniques which were available at the time when they were made, and the special channels in which the interests of psychologists were then directed. The 2 studies at about the turn of the century by Street (**38**) and Bonser (**4**) obtained from each subject a direct statement concerning the resemblance between himself and a friend in "temperament" and in "likes and dispositions." The 3 studies in 1922 and 1923 by Almack (**1**), Warner (**43**), and Williams (**45**) were concerned with chronological age, mental age, and IQ. As various standardized measures of personality traits appeared, resemblance in these traits became the object of investigation, beginning with Wellman's (**44**) use of the Marston extroversion-introversion scale in 1926. Statistical analysis in most of the studies prior to 1927 is not carried as far as in the later ones. Prior to 1927, Almack (**1**) was the only one to use the correlation technique. Of the 7 studies previous to 1929, 6 were concerned with the elementary or high school age, and 1 with ages 17 to 21. Beginning with 1929, 6 of the studies have been concerned with college friendships, 5 with children and adolescents, and 3 with preschool children.[4]

[4] This figure includes merely the 3 studies which deal with other factors than chronological age and sex in relation to choice of companions.

Preschool Friendships. During the preschool age, social contacts between children increase with increasing years, according to the observations of Beaver (**2**), Moreno (**27**), Parten (**30**), Salusky (**34**), and Zaluzhni (**50**). Where the age range in the group is wide enough to permit considerable range of choice, it has been found that children tend to select companions of similar age (Chevaleva-Janovskaja, **8**; Challman, **7**; Green, **15**). Where the age range is 18 months or less, as in the groups observed by Hagman (**16**) and Hubbard (**18**), chronological age does not appear to influence the selection of companions within the group. Cleavage on the basis of sex was found by Hagman (**16**) to be absent in a two-year-old group but present in a four-year-old group. Chevaleva-Janovskaja (**8**) likewise found that the tendency to form unisexual associations increased with age. Challman (**7**) reports a marked tendency for preschool friends to be of the same sex.

The 3 studies which have considered other factors than chronological age and sex in relation to choice of companions are those by Challman (**7**), Hagman (**16**), and Hubbard (**18**). Resemblance in mental age appeared to be related to companionship in Hubbard's group of 18 children aged 21 to 39 months, and among Hagman's 18 two-year-olds; and the same holds true for IQ among Hagman's two-year-olds. Resemblance in mental age and IQ and extroversion were found to be unrelated to paired companionship among Hagman's 24 four-year-olds and in Challman's group of 33 children ranging from 27 to 59 months in age. Among two-year-olds, Hagman obtained a correlation of $-.476 \pm .134$ in extroversion when each child was paired with his most frequent companion. Similarity with regard to attractiveness of personality (adult ratings) was found by Challman to be unimportant, and Hagman found the "Social Stimulus Index" similarly unimportant among her two-year-olds, though of some significance among the four-year-olds. In both age groups the most important factor related to choice of companions found by Hagman was similarity in Social Reaction indices. Challman similarly reports that resemblance in social participation was the most important factor in friendships between girls, and that resemblance in sociality apparently carries some weight in determining friendships in either sex. Hubbard, however, found that similarity in social participation was unimportant in her group. This inconsistency may be due to a difference in methods of measurement or to the small number of cases in all the groups compared. Similarity in degree of physical activity bears some relation to friendships in both sexes, according to Challman.

Friendships at the Elementary and High School Level. At the elementary and high school level resemblance between friends in intelligence, especially mental age, is reported by Almack (**1**), Warner (**43**), Williams (**45**), Furfey (**13**), Jenkins (**20**), Partridge (**31**), Seagoe (**37**), and Pintner, Forlano, and Freedman (**33**). Socioeconomic status of parents was found by Jenkins (**20**) to be of great importance. The influence of propinquity appears strongly in studies by Furfey (**13**), Seagoe (**37**), and Pellettieri (**32**). Resemblances rather than differences between friends were found in likes and dispositions (Bonser, **4**), social maturity (Furfey, **13**), play interests (Jenkins, **20**), and a number of personal characteristics, notably athletic ability, courtesy, and cleanliness (Seagoe, **37**). Data presented by Wellman (**44**) reveal less friend resemblance in extroversion than in scholarship (girls) or IQ (boys). Pintner, Forlano, and Freedman (**33**) found friend correlations near 0 on measures of cultural attitudes, ascendance, extroversion, and emotional stability.

Friendships at the College Level. In connection with a study in moral education, Street (**38**), in 1898, found that out of 189 persons (mostly girls between the ages of 17 and 21) who replied to his questionnaire, 46 reported that they were attracted by persons of opposite temperament, 43 by similar persons, 50 gave no clue, and 50 confused the issue.

Five more studies of college friendships are summarized in Table II. It is evident that similarity rather than dissimilarity is the rule, though the correlations are low or moderate. This general trend is in harmony with that of questionnaire replies received by Bogardus and Otto (**3**) from 138 men and 162 women students concerning interests, attitudes, and abilities.

The Criterion for Friendship. The studies of resemblance between friends present a problem which is not found in the studies of marriage partners: the matching of the subjects in pairs. The criteria for friendship have been of 2 main sorts: observed association, and the designation of one friend by the other. Each of these is open to a certain amount of criticism. Friendship may be inferred from the fact that 2 persons are frequently seen together, but it is not thereby proved. As for designation of one friend by another, the unsatisfactoriness of this criterion is indicated by the fact that A may name B as his " best friend," but B may name K and make no reference

TABLE II

SUMMARY OF STUDIES OF RESEMBLANCE OF COLLEGE FRIENDS

Characteristic	Investigator	N of Pairs	*r*	Remarks
Socioeconomic Status (Sims)	Flemming (11)	23	.20	Men.
		45	.24	Women.
Intelligence (Thorndike)	Garrett (14)	110	.21	Men.
" "	Flemming (11)	21	.12	Men.
		45	.51	Women.
" (Army Alpha)	Flemming (11)	48	.14	Men.
		60	.11	Women.
" (Thurstone)	Vreeland & Corey (42)	15	—.30±.14	Both members of each pair were of same sex, but some pairs men, some women.
Social Intelligence (G. Washington Univ.)	Garrett (14)	110	.27	Men.
" " " " "	Flemming (11)	18	.44	Men.
		11	.54	Women.
" " " " "	Vreeland & Corey (42)	30	.40±.10	See note above.
Neurotic Tendency (Thurstone)	Vreeland & Corey (42)	30	.33±.11	" " "
Introversion-Extroversion (Laird)	Flemming (11)	39	.56	Men.
		59	.13	Women.
Pleasingness (Rating)	Flemming (11)	48	.34	Men.
		61	.53	Women.
Emotional Steadiness (Rating)	Flemming (11)	48	.17	Men.
		61	.20	Women.

TABLE II—*Continued*

Characteristic	Investigator	N of Pairs	*r*	Remarks
Emotional Expressiveness (Rating)	Flemming (**11**)	47	.15	Men.
		61	.03	Women.
Social Adjustment (Rating)	Flemming (**11**)	48	.38	Men.
		61	.42	Women.
Attitude Toward War (Droba)	Vreeland & Corey (**42**)	30	—.016±.12	See note above.
Public Opinion Test (Watson)	Vreeland & Corey (**42**)	30	.09±.12	" " "
Opinions on Foreign Affairs, Race, Religion, Economic and Political Questions	Winslow (**49**)	86	.24±.07	Same-sexed pairs, both men and women.
			.35±.09	Men.
			—.10±.09	Women.
Will-Character (Rating)	Cattell (**6**)			62 students were asked each to name 1 intimate friend from the group, and 1 person disliked. Some named more than 1. Number of pairs is not stated. Differences in amount of resemblance between friends and enemies were small and of low reliability, but showed that similarity favored compatibility except in perseveration.
Surgency of Temperament	"			
Perseveration	"			
Fluency of Association	"			

to A.[5] The most satisfactory evidence would appear to be found where A and B name each other as best friends. Probably each criterion assures some degree of friendship, however, or indicates that in some way the pair are related more than by a random matching. Moreover, even under the same criterion not all the friendships will be equal in intimacy.

Table III classifies the studies according to the criterion for friendship which was adopted by the investigator, and also indicates the ages of the subjects. In only one study, that by Seagoe (37), were all the pairs mutually named and free from overlapping. The figures which we quote from this investigator show what a wide net was cast in order to obtain a group of 29 pairs who twice designated each other as first choice.

COMPARISONS AND CONCLUSIONS

In view of the facts that the present measures of personality traits still leave something to be desired, that the groups measured have differed widely in age and in degree of homogeneity, that the criteria for pairing of friends have not been entirely satisfactory, and that there were different degrees of compatibility among the husbands and wives, such consistencies as appear among the results of these studies are all the more worthy of note.

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[5] The question whether or not choices are mutual receives especial attention in the sociometric studies of Moreno (27). Pupils in an elementary public school and a private preparatory school were asked to indicate whom they would like to have sit near by, and girls in the New York State Training School were asked whom they would prefer for housemates. Persons were found who "like stars, capture most of the choices, others forming mutual pairs, sometimes linked into long mutual chains or into triangles, squares, or circles, and then an unlooked-for number of unchosen children." In one "social atom," A might be attracted to B, C, D, E, F, and G; B, C, and D might reject A; and G might be the only one positively attracted to A.

Moreno's approach differs from that in the other investigations included in the present review. The aim was to study the network of psychological currents in a group with regard to its bearing on the group adjustment. Individuals were interviewed as to the reasons for their attractions or repulsions. There is little statistical analysis of the factors which led to "clicking" or the reverse. Among the generalizations from some of the interviews reported in a supplementary section by Jennings (21) appear the statements that "children appear to choose associates according to attributes necessary for the joint pursuit of common aims" (third grade); "the motivations are often based on similarities of traits, physical and mental, of social standing, and of interests in common pursuits. . . The rejections are . . . based largely on differences, physical and mental" (fifth grade); "occasionally choices are made motivated by complementary attributes" (seventh grade).

Throughout all the traits and the range of ages the correlations

TABLE III

CLASSIFICATION OF STUDIES OF FRIENDS ACCORDING TO THE CRITERION FOR FRIENDSHIP

A. *Observed Association.*

- Systematic Recording of the Number of Times the Subjects Were Seen Together.
  - Challman (preschool)
  - Hagman (preschool)
  - Hubbard (preschool)
  - Wellman (elementary school)
- Self-selected Groups at Summer Camp.
  - Partridge (adolescent boys)
- Close Companionship Recognized by Others.
  - Vreeland & Corey (college)
  - Warner (elementary school age, boys' gangs)

B. *Designation of One Friend by Another.*

- Without Regard for Mutuality.
  - Almack (elementary school)
    - First choice for inviting to a party.
    - First choice to help in work.
  - Bogardus & Otto (college): "chum."
  - Bonser (high school): "chum."
  - Cattell (college): "one intimate friend." (Some named more than one.)
  - Flemming (college): "best friend."
    - Out of 61 women there were 38 mutual designations.
    - Out of 48 men there were 18 mutual designations.
  - Garrett (college): "one best friend."
  - Pintner, Forlano, & Freedman (elementary school)
    - Each child listed his 3 best friends in order of preference. In 3 out of 4 school groups, the friends named had to be from the child's own grade.
  - Street (ages 17 to 21 years)
  - Williams (delinquent adolescents): 2 "chums" named by each.
  - Winslow (college)
    - Each student who answered the questionnaire also gave it to 1 friend.
- Mutual Naming.
  - Furfey (Boy Scouts)
    - 35 boys each named the ones he most liked to play with. 62 mutual pairs were found.
  - Partridge (adolescent boys)
    - Each boy named 3 camp chums. Only mutual pairs were included.
  - Seagoe (elementary school)
    - First choice for inviting to a party.
    - Out of 823 children, 115 choices were mutual. Group I.
    - Out of these 115 pairs, 29 chose each other again 1 month later. Group II.

C. *No Statement Regarding Criterion.*

- Jenkins (junior high school)

between the paired scores of friends or marriage partners have been positive with very few exceptions.

At all ages, with the possible exception of part or all of the preschool period, a tendency to resemblance in intelligence was found. Where the population sample under consideration showed a relatively narrow range—notably in the case of college friends and in Schiller's group of married couples—the correlations in intelligence were low; otherwise they were moderately high. This influence of the variability of the group upon the size of the correlation for mental age appears clearly in the study by Pintner, Forlano, and Freedman (**33**). When Grades 5 to 8 were included in the group, the correlations ranged from .45 to .62 in various schools and for varying friend combinations. When a single-grade group was under consideration, the correlations ranged from .05 to .17. These data suggest that among children a large part of the friend correlation in mental age is due to a related chronological age factor. In the adult studies which cover a wide social-economic range it would be desirable to know how much correlation in intelligence scores would remain if the social-economic factor were partialed out.

In traits of temperament, the correlations at most ages have tended to run considerably lower than in intelligence, though they are still positive in the great majority of cases. The question may be raised whether the low and unreliable coefficients are due to a lack of a definite trend with respect to resemblance, or whether they are partly a by-product of the unreliability of the available measuring instruments in this field. It is in the field of temperament that the one negative correlation which approached reliability appeared: the correlation for extroversion in a group of 15 two-year-olds.

In attitudinal traits appear the greatest differences between the results for the several age groups. At the elementary school level, Pintner, Forlano, and Freedman (**33**) found correlations in measures of cultural attitudes to be near 0. Between husbands and wives, however, correlations in attitude scores are among the highest that have been found in any type of trait. The fact that they are lower between college friends may be due to an age factor again; the data are meager, however. The field of attitudes and interests appears to be one of the most promising approaches to the study of marital compatibility, judging from Kirkpatrick's (**25**) success in discriminating between the happily and the unhappily married by means of his measures of community of interests, and from some of Terman and Buttenwieser's results (40, 41). Further studies of this type are desirable.

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BOOK REVIEWS

SKINNER, B. F. The behavior of organisms. New York: Appleton-Century, 1938. Pp. ix+457.

Skinner proposes a system for the convenient formulation of behavioral data, and then proceeds to describe experiments which test the system. The book represents the culmination of a program of research originally projected in a semihistorical doctoral dissertation at Harvard on the concept of the reflex. The experimentation, concerned almost exclusively with the lever-pressing activity of rats, began to appear in 1930. The book summarizes previous reports and adds new data. In order to estimate the success with which the author achieved his purposes, tentative answers to three questions will be attempted: (1) What are the chief characteristics of the system which he proposes? (2) Of what significance are the experimental findings, both as a validation of his system, and in relation to the problems of psychology conceived in other ways? (3) Is the system as formulated and supported experimentally likely to become a competitor to other existing systems of psychology? These are difficult questions, which cannot be answered confidently. Yet to ask any less significant questions would be to underestimate the task which the author set himself.

1. *The Nature of the System.* Skinner proposes what is strictly a science of behavior, therefore neither a mental science nor a neural science. His system is in this respect in keeping with current trends as represented, for example, in the writings of Guthrie, Hull, Lewin, and Tolman. In none of these systems is there recourse to neuroanatomy. Skinner takes a firm stand in favor of descriptive positivism, against hypotheses. "A purely descriptive science is never popular. For the man whose curiosity about nature is not equal to his interest in the accuracy of his guesses, the hypothesis is the very life-blood of science" (p. 426). "Deductions and the testing of hypotheses are actually subordinate processes in a descriptive science, which proceeds largely or wholly without hypotheses to the quantitative determination of the properties of behavior and through *induction* to the establishment of laws" (p. 437). In this he is, of course, outside the trends currently popular in psychology.

Since the structure of a descriptive system is determined by its subject matter, it is pertinent to inquire how the subject matter is selected. It is evident that two influences have directed Skinner's choice of representative behavior. In the first place, he believes that the reflex is the analytical unit which makes possible the scientific investigation of behavior (p. 9). The reflex is not to be thought of in neural terms, however, but is to be defined as a correlation between stimulus and response. The choice of the reflex as the analytical unit determines the general formulation of the 'laws' which include after-discharge, temporal summation, refractory phase, facilitation, inhibition, conditioning, extinction, and so on. It is evident that the laws are not discovered or formulated entirely *de novo,* but derive largely from Sherrington, Magnus, and Pavlov. They are all redefined operationally to apply to behavior without neurological implication, and as so defined they are not the laws of spinal reflexes. As stated, they do not appear to the reviewer to be laws at all, but collections of variables probably correlated in such ways that laws might be looked for. To describe them as laws of behavior is like speaking of a 'law of moisture' or a 'law of sunshine' as laws of growth at the stage when little more is known than that moisture and sunshine favor growth. The 'laws' do, however, direct the inquiry, and hence are surrogates for hypotheses. The choice of the rat's lever-pressing for food as the representative reflex was probably dictated by the desire to show that precise relationships much like those of the neurologists could be validated within behavior which physiologists would not be tempted to call reflex.

It would be a mistake to give the impression that Skinner makes a careless use of analogy in calling a rewarded act a reflex or in adopting the physiologist's names for the laws which describe this act. His is a formal and sophisticated system, and when he does violence to the contemporary socially accepted concept of the reflex, he knows very well what he is doing. Definitions are given with extreme care.

The real significance of the selection of a rewarded act as the representative behavior apparently became clearer to Skinner as the experiments progressed, for one of the more important distinctions did not appear in the published reports until 1937. This is the distinction between *respondent* behavior, which, like an ordinary reflex, is *elicited* by a precise stimulus, and *operant* behavior, which is not elicited by identifiable stimuli but may be said to be *emitted.* This is the behavior, sometimes called random or spontaneous, important in trial-and-error situations such as that which Skinner studies. Respondent behavior is significantly correlated with antecedent stimuli; the relations into which operant behavior enters are different. It is operant behavior which is strengthened when lever-pressing is rewarded. It does not matter what caused the rat to depress the lever the first time; once the operant response has produced food, the operant is strengthened. When operant behavior is correlated with a stimulus, the situation always involves discrimination. The discriminated stimulus is really only a cue or occasion for the behavior, not a true stimulus to elicit the behavior. The distinction between respondent and operant has been implicit in Thorndike's work all along, but it did not become explicit because the use of 'situation' to cover discriminated as well as eliciting stimuli permitted a spurious application of the stimulus-response formula. This clear distinction is perhaps Skinner's most significant conceptual contribution. He hopes to correct the disproportionate emphasis upon respondent behavior by basing all of his work on operant behavior. Having formulated laws after the pattern of reflex physiology, Skinner's problem is to validate and quantify the laws within operant behavior.

The respondent-operant distinction is an important one in setting up two types of conditioned reflex. Pavlov's variety is based on respondents, and because the correlation of response is with substituted stimuli, this is designated as Type S. Skinner's variety strengthens a response (an operant) by rewarding it, and to emphasize the response this is designated as Type R. Actually, Pavlov's experiments are not pure illustrations of Type S, but for expository purposes a fairly stereotyped description of Pavlov's experiment is used by Skinner. The distinction between these types is that made earlier by Thorndike between associative shifting and trial-and-error.

2. *The Experimental Data.* The bulk of the book is devoted to experimental findings in which the rate of response in the lever-pressing situation is correlated with many pertinent variables: drive, reinforcement, nonreinforcement, delayed reinforcement, periodic reinforcement, discriminatory stimuli, and differentiated response. The data are presented chiefly in the form of 148 figures, most of which are reproduced kymograph records. Many uniformities are demonstrated, confirming the position that lawfulness may be found in this situation. The result is lawfulness rather than new or reformulated laws. It is difficult to determine within a positivist system just what level of generality constitutes a law. The laws formally stated before the experimentation is reported are not resummarized after the data have been discussed. It is to be supposed that they were found adequate. If this interpretation is correct, the laws were merely definitions of variables to be investigated, and experimental verification means not that the laws are proved or disproved, but merely that the variables chosen were convenient to direct inquiry.

The real quantitative laws are not, then, the laws formally stated, but the equations which fit the reported curves in each specific instance. There is a uniformity about the eating rate under standard conditions which may be expressed by the law that $N=kt^n$, where N is the number of pellets eaten in time t, with k and n appropriate constants. This is never specifically called a law, but it is as near to one as any relationship which Skinner reports. There are many relationships of this kind which are important contributions both to the factual knowledge of behavior and to methodology in behavioral investigations. One or two illustrations may be added to indicate the richness of the data. After a single reinforcement (once receiving a pellet following lever-pressing), there follow a number of responses to the lever although pressing is no longer reinforced. This yields a characteristic extinction curve. Probably no other conditioning method provides as sensitive an indicator of the result of a single reinforcement. The concept of the 'reflex reserve' emerges, to be distinguished from momentary strength. The 'reflex reserve' is the potential number of responses to be made without further reinforcement: it might be called 'resistance to extinction,' in more conventional terminology, although this does not define it adequately. A further demonstration of considerable methodological interest is provided under the concept of 'periodic reconditioning.' When responses are reinforced every three minutes or every six or nine or twelve minutes, a characteristic uniform rate of responding results, represented graphically by a straight line of slope varying with the interval. The number of responses under standard conditions is relatively constant, say eighteen per reinforcement. This value is characterized as the 'extinction-ratio.' Because of the linearity of the response curve within periodic reconditioning, it is feasible to use this curve to test the influence of other variables, such as differences in drive. Periodic reconditioning is not to be confused with reinforcement at a fixed ratio. That is, if every tenth response is reinforced, the result is not uniformity of response, but acceleration. Ratios as large as one reinforcement for every 192 responses are reported; under these circumstances very high rates of responding occur, showing positive acceleration between reinforcements similar to that which would be predicted from Hull's goal-gradient hypothesis. These few specimens can only suggest the great variety of relationships which have been explored, many of which are distinctly new and should be assimilated to the body of psychological knowledge.

3. *Estimate in Relation to Other Systems.* In choosing a representative sample of behavior, Skinner has been restricted by his bias in favor of the reflex. Having made the choice for operant behavior against respondent behavior he believes himself to have chosen more representative behavior than that usually chosen, *i.e.* by Sherrington and Pavlov (p. 438). Although he is outspoken in his denunciation of a science of behavior which subordinates itself to neural science, he is more conspicuously aware of neurologists and physiologists than of psychologists. Respondent behavior is, after all, not very prominent in Ebbinghaus, Freud, McDougall, the later Thorndike, Gestalt, and in many other behavioral systems less physiological than Skinner's. Had he chosen to modify their systems, rather than the systems of those working with reflexes, he might have developed an entirely different program, based on different representative behavior. It is interesting in this connection to note that he devotes a whole chapter to clarifying his service to neurology, with which he has broken, but he devotes only three pages specifically to the systems of other psychologists with which his work is coördinate. The statements about Lewin, Hull, and Tolman on these pages are intelligent, but cursory. Tolman's system is recognized as the nearest relative. Thorndike, another close relative, is ignored in this comparison.

It is unfortunate that Skinner did not do his readers the service of relating his system in greater detail to the experimental data of other investigators. His own comment is significant: "There is no implication whatever that this is the only important work that has been done in the field, but simply that I have had little luck in finding relevant material elsewhere because of differences in basic formulations and their effect on the choice of variables to be studied" (p. 47). If Skinner has been unable to relate his work to that of other investigators, how can a reader, coming fresh upon this new body of material, be asked to make the transitions? The difficulties in making the extensions of the system may result in the book's being less useful, and perhaps less influential, than it ought to be.

That Skinner's task of going beyond his own experiment would not have been insurmountable is evident through the studies now beginning to appear from Hull's laboratory, in which Skinner's situation is used, but the results of which are reported in accordance with more familiar conventions. The expedient of adding a second lever coördinates his situation with choice-point behavior, so important in other systems. It may be that these and related studies will result in bringing to Skinner's work the attention which it deserves.

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OGDEN, R. M. The psychology of art. New York: Scribner, 1938. Pp. xviii+291.

In the early years of his academic life R. M. Ogden acquired an interest in things aesthetic at least partly through his contacts with W. A. Hammond and O. Külpe. In 1905 he published the *Esthetic attitude* and in 1907 he was the cotranslator with Max Meyer of A. Hildebrand's *The problem of form in painting and sculpture.* Since that time he has published an occasional treatise on the psychology of the arts, the best known being his book, *Hearing,* which appeared in 1924. The scope of these offerings has not been narrowly confined but has dealt with such widely different problems as psychical distance, consonance, naïve geometry applied to painting and architecture, and schools of art. The present book can be considered as touching on all of these interests.

The title, *The psychology of art,* adequately indicates the book's contents only if one accepts a rather restricted view of psychology. The treatment is observational and quantitative, but rarely experimental in the sense in which modern psychologists employ the term. Of the few experimental findings noted, the majority are to be found in the auditory sections. The only American psychologists to whom reference is made are G. T. Buswell, R. C. Givler, and H. S. Langfeld, and the German-Americans included are W. Köhler and K. Lewin. The contents reveal the interests of a well-trained psychologist who has a tremendous interest in and a large knowledge of the arts. In consideration of the treatment, a more adequate title might have been *The arts as a psychologist sees them.*

The author defines the aesthetic as "a felt behavior, the pattern of which lacks discernment." The artistic is "the perfection of means to ends, a perfection which becomes artful only as the means themselves become an end" (p. 16). As a Gestalt psychologist, Ogden warns against too static a type of aesthetic analysis. Birkhoff's well-known M, for example, is far too static for a place in the author's system. Every work of art is a figure-ground pattern which is nonenumerative.

After an introductory section, two chapters are devoted to the problems of music. The material is essentially, with a few extensions, what is to be found in Max Meyer's *The musician's arithmetic.* Ogden's treatment is rather uneven. Valuable space needed to amplify the description of difficult material is occasionally devoted to what is essentially grade-school material, *e.g.* over a page is devoted to a description of the several sorts of rests and notes. On the whole, however, the treatment is concise and fair.

Poetry is discussed under the major headings of literary art and prosody. The subheadings are: poetry and prose, the nature of literary art, linguistic sound, rhyme, assonance, alliteration, rhythm, literary style, the poetic foot, poetic metre, modern poetry, poetic inspiration, and literary forms. The material, in the main, is far-removed from the psychological laboratory. Ogden's treatment, therefore, is forced to be largely that of the professor of English. The chapters, nevertheless, make interesting reading.

The five chapters devoted to visual art are profusely illustrated with photographs and figures. The more important of the currently accepted art principles are described and many psychological findings are shown to bear directly on the problems of the arts. Attention is paid to the tectonic arts as well as to drawing, painting, and sculpture. The author dislikes the currently popular "modern house." To justify this dislike he has presented a photograph at which even the enthusiasts for modern housing will shudder. His choice of a photograph to illustrate the modern factory building (which, incidentally, he likes) is a much happier one.

Throughout these chapters the author's 'gestaltish' inclination is shown by frequent references to figure and ground, by the mention of "strong figures," by a lack of interest in preferences for isolated colors and forms, and by the quest for functional entities. The discussion of the last-mentioned quest furnishes what is perhaps the most intriguing portion of the book. Art objects are analyzed in an attempt to learn whether they show static or dynamic symmetries. Naïve geometries are assumed to have operated in the construction of the famous art objects of antiquity. In speaking of architecture Ogden says: "It is surely not a matter of chance that measurements of the whole permit an analysis of its members into subtle geometrical proportions" (p. 239).

Many aestheticians and psychologists will not hold with Ogden that geometry has these intimate connections with the arts. They may see, for example, little relation between our high regard for the Parthenon and the fact that its Euthynteria contains two squares and two root-five rectangles. Yet it is the reviewer's guess that they cannot but be fascinated by Ogden's discourse on golden sectors, root rectangles, Pythagorean stars, whirling squares, and all the other geometric oddities.

The last section is devoted to an appeal for eurhythmics, for "the use of right rhythms in postural behavior" (p. 272). As Ogden conceives the issue, our pedagogy should retrieve, if possible, the rhythmic procedures of classical Greece. Dalcroze's more narrowly conceived system of eurhythmics furnishes a step in the correct direction. To this should be added the rhythmic approach to vocalization and verbalization. "Aesthetic pleasure and efficient performance go hand in hand" (p. 272).

The Carnegie Corporation of New York should be thanked for financing Ogden's *The psychology of art*. Except for the section on music, the Ogden book overlaps very little the other modern volumes devoted to the psychological aspects of aesthetics. It is packed with interesting observations and speculations. For these reasons it should find its place on the book shelf of every psychologist who has an interest in some one of the arts.

PAUL R. FARNSWORTH.

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FREDERICK, R. W. How to study handbook. New York: Appleton-Century, 1938. Pp. xxviii+442.

So many crimes have been committed in the name of secondary school handbooks on "How to Study" that one has come to look askance at any volume bearing this title. Because volumes of this sort have issued chiefly from the hands of well-intentioned but uninformed folk, the appearance of a carefully written handbook which bears the earmarks of firsthand acquaintance with cognate psychological researches is an event worth noting. Frederick's manual unquestionably belongs in the latter category. Instead of presenting speculative 'rules' proclaimed as true by a man of experience, Frederick has produced a series of discussions which are uniformly aligned with the literature of experimental psychology. Furthermore, he has, without loss of dignity, written on a level suitable for secondary school students. Some portion of the credit should go to W. H. Burton for the editorial assistance acknowledged by the author, but the coherence of the final product leaves no doubt as to the soundness of the groundwork itself.

The volume opens with a dozen brief chapters on reading, which occupy a total of ninety-eight pages. The exposition is clear and thought-provoking. One might wish for a more extensive treatment of remedial measures, but one cannot quarrel with the correctness of the discussion that is presented. Indeed, there are few college students who would fail to profit from a careful perusal of this section.

Later chapters deal with such topics as listening in class, gathering materials for reports, making notes, and dealing with examinations. The chapter on experimentation seems to this reviewer to present too simple a view of the case, and the chapters on thinking might with profit have been carried beyond the conventional limits. The author does occasionally slip into careless use of such psychologically flavored terms as 'unconscious' and 'subconscious' and he does go into details which have nothing to do with studying and which only serve to interrupt the march of the exposition. These shortcomings, however, are not of great importance, and they are more than nullified by the empirical tenor and logical coherence of the volume. It may be enough to say that many psychologists will probably be pleased to see it in the hands of the teachers of their own offspring.

JOHN G. JENKINS.

University of Maryland.

REID, A. C. Elements of psychology: an introduction. New York: Prentice-Hall, 1938. Pp. xix+409.

The appearance of a new textbook in general psychology is hardly a novelty these days. Hall's concern in the period of the nineties about the "multiplication of textbooks" would appear to be more germane if expressed today than in Hall's own day. It is not easy to classify current textbooks in general psychology. Undoubtedly, however, it would be fair to state that many texts show the influence of functional and dynamic concepts—perhaps the dominant psychological mode in America. Several current texts utilize behavioristic concepts and Gestalt interpretations.

A great many—perhaps a large majority—are undeniably eclectic in treatment. There appears to be a slight tendency to write psychology texts down to the level of the great mediocrity. This trend is hazardous for psychology or any other science. (The above statement is not to imply, however, that there may not be a field for books written in a humanistic vein.)

Taking as a point of reference the current modes of psychological treatment, Reid's psychology appears to be anachronistic; for " Elements of Psychology " is a thoroughgoing introspectionism. The writer has undoubtedly been influenced very greatly by the teachings and writings of the late Professor Titchener, and the pattern of the book resembles closely the Titchenerian model. Since mind is defined in terms of conscious content, it will be obvious that the text omits a discussion of many topics that are of current interest in psychology, such as perceptual-motor learning, motivation, intelligence, and personality.

Of the 379 pages of text, 130 pages, or slightly over a third of the book, are devoted to the topic of sensation, including intensity relations. Other orthodox topics, from the introspective point of view, are, of course, image, affection (sensation, image, and affection are regarded as mental elements), perception, association and memory, attention, emotion, and action.

The material of the book is systematically organized. The style is clear and concise, and the treatment is serious and dignified. For any teacher who might wish to introduce psychology from an introspective point of view, the reviewer would recommend the book without equivocation. So far as the reviewer was able to detect, the book is relatively free from errors. However, he was disturbed by the definition of 'retroactive inhibition': " Retroactive inhibition is the condition in which the process of forming associations seems to stop some time prior to the moment at which one's experiences cease." The discussion following this statement seems to indicate that the author has in mind what is generally referred to as anterograde amnesia. From an etymological standpoint, this might be a possible usage of the term; however it has come to have a technical meaning in psychology which is quite different from this, and which has widespread acceptance.

PAUL L. WHITELY.

Franklin and Marshall College.

POWERS, F. F., MCCONNELL, T. R., TROW, W. C., MOORE, B. V., & SKINNER, C. E. Psychology in everyday living. Boston: Heath, 1938. Pp. x+511.

The purpose of the authors in writing this volume was " to meet the need of the college student for a well-integrated, modern text which will materially contribute to his ability to manage his own life, and to the realization of certain other fundamental purposes of a general liberal education." This book is intended for those " students who, for the most part, will never become professional psychologists." Hence, " such concepts as managing one's own life, personality development, planning a career, socialization, learning, and other practical applications have been stressed." In the words of the authors "the volume is not just another traditional textbook."

An attempt is made not only to present "the facts of empirical psychology, but also to show their implications where possible." Therefore, to this end, the book, consisting of twenty-one chapters, is divided into five parts: (1) "The Nature of Psychology," (2) "Understanding Ourselves," (3) "Adjustment to College and Life," (4) "Learning and Study," and (5) "Applications of Psychology."

Chapters I and II deal with topics that are usually considered in the introductory chapters of texts written for beginners. These chapters, however, are flavored somewhat with practical suggestions, and, the writer thinks, should prove to be rather stimulating to the beginner. Many of the traditional topics usually treated in a text intended for beginners are omitted. Others are briefly treated in Chapters III to IX, inclusive. The discussions of the determinants of behavior, the physiological and functional principles, and types of behavior are inadequate. The treatment of receptors, connectors, and effectors is hardly more than an outline of these parts. Some valuable and practical suggestions are offered in the chapters dealing with personality and adjustments. Although the treatments of emotions, intelligence, motivation, and individual differences are simple in nature, they are modern in substance and thought-provoking.

There is a tendency on the part of the authors to do a lot of listing and enumerating. Throughout the entire book discussions are brief. Hence, the writer thinks that perhaps beginning students who use the book as a text will be apt to learn *more about* psychology and *less of* psychology. However, this approach to a study of the science may be the better one. If and when a student learns a lot about a science, his interests usually become more profound in it, and he is stimulated to further study in that field. Often, the first course taken in a field of study furnishes the basis for a decision, on the part of the student, either to take more courses in that field or to drop the study there. In that case, the comprehension of the real nature and subject matter of psychology might well be left for succeeding courses.

The materials discussed in Part Three, comprising Chapters X, XI, and XII, rightly belong to the fields of social psychology, vocational guidance, and matrimonial guidance. The brief consideration of problems in these fields might well be extended. While the treatment is brief, many timely suggestions are offered that should prove to be most valuable.

The discussion of the learning process, constituting all of Part Four, continues for five chapters. This consideration, including a discussion of the formation of study habits, is the most exhaustive treatment in the book. The authors avoid all controversial topics and theories in this field. Also, they do not advocate any new theories and short cuts in the learning process.

Part Five is a treatise on applied psychology. The fields of application here considered are business, industry, law, politics, religion, medicine, and education. It is surprising to one to learn how much practical psychology is to be found in the volume. Relatively few footnotes are employed, but copious references are to be found in connection with the various chapters. In addition, there is an appendix containing a long list of supplementary books.

OSBORNE WILLIAMS.

University of Florida.

STRANG, R. An introduction to child study. (Rev. ed.) New York: Macmillan, 1938. Pp. xv+681.

"It is hoped that this book will prove useful to individual parents . . . ; to parents' clubs, county demonstration groups, and other organizations of adults; to parent-teacher associations; to advanced high-school students in home-economics classes; to teachers . . . ; to students in liberal arts colleges . . . ; and to students of psychology . . . ; as well as to students in teachers' colleges, normal schools, and nurses' training schools, for whom the book is primarily intended." Dr. Strang's statement of the composite reader-group for whom she is writing explains to the critical reader the essentially elementary character of the book. She has succeeded in keeping the style and treatment simple enough for the comprehension of high school students and of so-called "average" parents. By so doing she has undoubtedly oversimplified to the point that the work has lost some of its value as a textbook for the more advanced groups for which she also designed it. This is inevitable; and it is probable that the greater need today is for a book which will meet the needs of the larger group of less specialized students, elementary teachers, nurses, and parents.

In her selection of material the author has been no less restricted than in delimiting her audience. Obviously, it is conceivable that, in its broader scope, "child study" does include material from a great variety of approaches, including those of medicine, pediatrics, anthropology, and nutrition, as well as those of psychology, education, and "child training." However, it is a Herculean task to attempt to unify material from so great a variety of sources, and it is not surprising that some sections of the book are little more than compendia of useful information.

In developing her material, Dr. Strang has followed a strict chronological order, from the prenatal period through adolescence. Each section is divided into chapters on development, how the child learns, special problems of the period, and suggestions on how to study the child. There is evidence of thorough familiarity with the literature of the subject, although on occasion the tendency to simplify leads the author to draw somewhat more definite conclusions than those of the original study. Each chapter is followed by a list of "questions and problems for class discussion or study groups"; each section carries an annotated bibliography of standard books in the field. From time to time there are blanks in which a mother may record the development of her own child, and compare this with the norm. The appendix contains height-weight tables for children from one month to twelve years of age, and a summary of play materials for children from birth to eighteen years of age. (This latter was prepared by Dr. Katherine B. Greene.)

In spite of the various individual excellences of the book, it is unfortunate that the general impression on the reader is that of a handbook of helpful hints, which might have been compiled from daily articles in a newspaper column. For the lay mother it probably has a certain value which might justify giving it a place on the shelf between the cookbook and the *World Almanac.*

HELEN MARSHALL.

Stanford University.

JUNG, C. G. Psychology and religion. New Haven: Yale Univ. Press, 1938. Pp. 131.

Dr. Jung works from the facts which his clinical experience discovers to broad and far-reaching hypotheses which, in turn, largely control his clinical methods—an entirely proper practice which is used by all scientists. Dr. Jung, no more and no less than others, faces the danger that facts which support his basic thesis are more easily discerned than those which apparently oppose it. Since the most fundamental hypothesis which Dr. Jung accepts is distinctly different from those accepted by the majority of psychiatrists, psychologists, and students of religion generally, his Terry Lectures for 1938 are somewhat difficult to read appreciatively.

The basic thesis which Dr. Jung accepts is that "self" is the totality of conscious and unconscious existence, a summation of individual and racial experience of which the latter is the more important both in bulk and in dynamic quality. Every individual experience contains something unknown, since some of its content comes from a psyche more complete than consciousness. In other words, it is a mistake to consider the individual human psyche as a merely personal affair.

The unconscious portion of the self makes itself obvious only in special circumstances. In emergencies, for instance, unexpected, new, and strange instinctive forces appear. Character is amazingly changed. In neurosis much the same kind of thing occurs. There is a manifestation of a power and a meaning which is not yet understood but which has a devastating effect upon even highly rationalistic and intellectual men.

The modern mind has forgotten those old truths which speak of the death of the "old man" and the making of the new, of spiritual rebirth, and so on. The modern attitude is to look back upon the mists of credulity and superstition from which man has emerged with pride, forgetting that he carries all of the past in his unconscious. The reviewer at this point is uncomfortably uncertain whether or not Dr. Jung regards the disappearance of superstition and credulity as regrettable. But perhaps he means that since, in his view, these have to exist somewhere it is better that they exist on the conscious level.

Religious experience seems to have to do with the unconscious areas of self which are, now and then, faced consciously. Such confrontation is terrifying and from it man seeks refuge in dogma and ritual. That is to say, religion is a substitute which replaces the immediate experience of unconsciously retained racial history with symbols which are vested in creed and ritual authoritatively supported by an institutional church or by evangelistic fervor. Dogma represents the self more completely than any scientific theory since the latter expresses the conscious mind alone. Ritual, largely abandoned by Protestants, to their partial undoing, has always been a safe and pleasant way of dealing with the unaccountable forces of the unconscious mind.

Religion is a relationship to the highest or strongest value. That psychological fact which is the greatest power in any system is its Deity. Any religion rooted in the history of a people is a true exposition of its psychology; that is, of its fears, desires, and frustrations. This relationship is expressed in symbols, the likeness of which in different religious groups proves the existence of an archetypal image of Deity—not at all the same thing as proving the existence of Deity, but very important nevertheless.

There is more in this compact little volume than this. Dr. Jung is convinced that dreams, especially those with any repeated content, are revelatory of the unconscious territories of mind. Dreams, he says, are visible links in a chain of unconscious events. In his argument he works from dream analysis to the more general theories reported above. There is also an interesting description of the quaternity symbol and of what the author calls the "heretical attempt to improve on the dogma of the Trinity."

Psychology and religion is interesting and valuable after the reader has mastered a somewhat difficult style. When properly considered as phenomena of social psychology rather than as a *sacrosanctus* beyond the possibility of intelligent consideration, religion must be recognized as of enormous importance. Every attempt at fundamental analysis is welcome. Dr. Jung has made a contribution of importance to scholars, though one more easily understood by devotees of his variety of psychoanalysis than by those who are skeptical of the whole psychoanalytic approach. The book is not intended for and has little to offer one who may be termed 'the practical religious worker.'

George R. Wells.

Hartford Seminary Foundation.

Partridge, E. D. Social psychology of adolescence. New York: Prentice-Hall, 1938. Pp. xv+361.

In writing the *Social psychology of adolescence,* Partridge set for himself the task of surveying the fields of psychology and sociology as they relate to young people. He emphasizes the rôle of patterns of culture as determinants of behavior of the young individual; hence, adolescence is attributed importance more because of social implications attendant upon development than because of biological processes occurring at that age. The point of view adopted is that an understanding of the behavior of young people entails a study of the "total configuration of the individual as a part of a large and complicated social scheme."

Chapters I and II are introductory to the main treatment of adolescence. In them Partridge stresses again and again the *relationships* in the environment as they influence behavior. A field-theoretical approach, Gestalt method, is suggested as a means of attacking these relationships. The timeworn arguments in favor of Gestalt methods and the typical Gestaltian attacks on analytical psychology are given. This controversial matter might well have been omitted. It can contribute little to a lay reader, or even an elementary student in psychology; furthermore, the treatment of subsequent portions of the book is little influenced by it.

In the remaining chapters of the book, the social psychology of adolescence is presented. As one would anticipate, the physical and physiological facts pertaining to adolescence are not stressed. Recognition is given them, however, as influences upon the personal and social adjustment of the individual. The author discusses the nature of the individual with respect to his adaptability through learning and through the adoption of common ways of maintaining integrity: retreating, rationalizing, sublimating, etc. The social implications of individual differences in mental abilities and physical characteristics are then presented. Following this, the informal group is characterized and its influences on individual behavior are given. Adolescent leaders and their influences on individuals are ably discussed in the light of numerous investigations. The remainder of the book is a comprehensive treatment of the influences on behavior of the group, the sexes, the family, leisure time, the community, education, and factors which lead to delinquency and other abnormalities.

For use as a one-semester text on adolescence this book has many virtues and few defects. It is interesting to read and is readily comprehensible. While it contains few figures, graphs, and tables, it has adequate contemporary references given in footnotes and at the end of each chapter. The transition from chapter to chapter is orderly and coherent. The author does not deviate from the field of study, the process of acquiring social maturity. This coherence is due in part to his contention that few characteristics of development are peculiar to adolescence. Thus he recognizes that development is continuous and that processes beginning early in life affect behavior as a constant rather than as a periodic influence. The book should be more teachable than its contemporaries which treat separately and often unrelatedly the development of such aspects as physique, intelligence, emotion, motivation, morality, and personality.

JOHN B. WOLFE.

University of Mississippi.

GREGG, F. M. The psychology of a growing personality. Lincoln, Nebraska: Personality Press, 1938. Pp. xv+489.

In *The psychology of a growing personality,* Gregg presents a McDougallian-tinged view of psychology for character educators and nonacademic men and women. He discusses the general topics of introductory psychology, personality, and mental hygiene around an outline of "stages" of individual development: Babyhood, Dramatic Age, Big-Injun Age, Age of Loyalty, Mate-seeking Age, Romantic Age, Adulthood.

Much of the material of the book came from lectures which the author had given to parents, teachers, and lay audiences. Hence, it is not surprising that little experimental data are given and, further, that the bibliography is heavily weighted with popular and secondary source materials. Stories and anecdotes are used extensively as interest-holding devices and as vehicles for character education, as the author conceives it.

Readers of this book may gain improper impressions from it. They are told, for example, that conditioning from environmental influences begins at the moment of birth; that an IQ of 77 means that the individual possessing it is 77% as smart as the average boy or girl of his chronological age; that educational psychologists are environmentalists partly because they have had little background in neurology; that extreme behaviorism (present-day behaviorism is ignored) is undesirable and frequently leads an individual into sensuality; that there are seven psychological theories of why people behave, four theories of personality determination, ten schools of psychology. Too much stress is placed upon instinctive tendencies as explanations of behavior and upon precarious " stages " into which development of behavior has been classified.

JOHN B. WOLFE.

University of Mississippi.

BLUEMEL, C. S. The troubled mind: a study of nervous and mental illnesses. Baltimore: Williams & Wilkins, 1938. Pp. ix+520.

A common procedure for books dealing with psychopathology is to give rather scanty descriptive material accompanied by extensive theoretical interpretations. Bluemel reverses this emphasis and presents a wealth of excerpts from the behavior records of patients while devoting relatively little space to the exposition of views as to why they behave as they do. Furthermore, instead of dwelling largely upon cases of extreme mental derangement, he devotes about 70% of his book to the milder forms of psychoneuroses.

He takes up in order some fifty symptoms. His general procedure is to give a description of the symptom and then to illustrate from case records the several ways in which this symptom may appear. He does not clutter up his case descriptions with irrelevant items the way so many writers in this field do.

The manifold forms that psychopathic behavior may take are made exceedingly clear by the multiplicity of types which Bluemel describes. To those persons who have not had extensive contact with psychotic patients this book should be exceptionally illuminating. It should help to dispel the current lay notion that there is some common factor running through all mentally disturbed individuals.

There is no doubt that the author himself considers the greatest contribution of his book to be the application of the principles set forth to an understanding of psychopathic individuals who are at large and especially those in public life. He considers the aggressive psychopath who manages to gain some official position to be more dangerous than the more modest psychotic individual who keeps his maladjustments a private matter. The drive of the public-spirited psychopath may lead to social unrest, political upheaval, war, and revolution.

The book is not intended for psychiatrists or specialists but for those individuals who must deal with people and who might be helped in their social contacts by substituting understanding for intolerance. Because of this aim the book says very little about treatment and the methods of treatment that are suggested are usually of a very general nature. Bluemel gives some theoretical discussion of the way in which the principles of learning and conditioning may be invoked to explain functional mental abnormalities but does not fall into the error of attempting to trace all abnormalities to some such central factors as the libido, inferiority complex, Oedipus complex, instincts, or the like.

The reader who is not familiar with psychopathology may be rather confused after reading this book because of the great variety of behavior types which are discussed, but this confusion is probably more wholesome than the false assurance that comes to the reader of an oversimplified treatment of mental diseases. The facts in this field are complicated and Bluemel does not gloss over differences in any attempt to ride a hobby.

Although the greatest emphasis is given to the description of symptoms, the author does not leave the reader with the impression that the removal of symptoms is the essential aim of therapy. He shows very clearly that these symptoms are the result of more deep-seated difficulties and, while temporary measures are sometimes demanded, sound procedure involves a search for the more basic causal factors.

The person who knows nothing of psychopathology can read this book with interest and profit, and the expert in the field will find the book very refreshing.

JOHN J. B. MORGAN.

Northwestern University.

ADAMS, R. Interracial marriage in Hawaii. New York: Macmillan, 1937. Pp. xvii+353.

Dr. Adams' book on racial intermarriage has for a factual foundation census reports, supplemented further by annual reports of the Governor of Hawaii. The discussion of the various population trends, as shown by these figures, is based, however, on Dr. Adams' own observations and experience as professor of sociology in the University of Hawaii, and leads directly to an interesting interpretation of the customs and attitudes of the various ethnic groups resident there. It is primarily a sociological document, but contains considerable material of value to social psychologists.

The general outlines of the history of immigration to Hawaii are widely known, but not the details. The early coming of the first Polynesian navigators who crossed the immense open spaces of the Pacific, first to discover and then to populate the islands, is lost to view except through the shadowy media of myth and tradition. The whites were the next discoverers and the earliest nonnative settlers. Then came in successive waves the peoples who now make up the bulk of Hawaii's population: Chinese, Portuguese, Japanese, Koreans, Puerto Ricans, Filipinos—each group contributing its share to the racial composite.

Hawaii has been called many things: the Paradise of the Pacific, a racial laboratory, the crossroads of the Pacific, America's listening post towards the Orient—all designations more or less deserved.

From the human standpoint, an interracial poker game in which the whites have assumed the perpetual right to shuffle and deal the cards and call the turn would be, in the reviewer's opinion, as apt an analogy as any. That it is as friendly a game, with the players borrowing and lending among themselves—and occasionally taking a peek at their opponents' hands—is rather extraordinary. To those who are interested in these adjustments and an authoritative account of when the players arrived and what they paid to sit in the game, Adams' book will be a useful guide. Racial intermarriage was and is the outstanding feature of these group adjustments and determines more than anything else who can borrow from whom.

Space will not permit any extended review of the author's observations and interpretations of the acculturation processes in Hawaii, but as the Japanese are the most numerous group and represent the player about whose hand the dealer is most uncertain, we might pay some attention to the chapter which deals with their social organization.

The Japanese, in 1930, constituted about 38% of the total population and four years later this proportion had not shrunk. Meanwhile, the fact that 44% of the female population was of this ancestry does not indicate any decided drop in the future size of the group.

Probably the most interesting fact about this immigrant group is that, unlike the Americans, British, and Chinese, they have not intermarried freely with other peoples. Their outmarriages have been comparatively few. Adams explains this unusual restraint as being largely due to cultural considerations, a most important one being that Japanese marriages were arranged on the basis of equivalence of family status. This cultural attitude was preserved through the fact that the Japanese came in large numbers and thus were able to maintain their own social life with its customary sanctions. Adams remarks on their superior social morale and attributes to this their good record in the Territory. "Relative to numbers," he says, "there are fewer arrests and convictions, there are fewer juvenile delinquents, fewer who receive charitable aid, fewer insane, fewer who are mentally defective." The psychologist would, of course, suggest that at least some of these advantages are due not only to "superior social morale" but also to superior biological inheritance. Even their lessened tendency to crime and delinquency may also indicate superior temperamental qualities, and the incidence of mental defect certainly is not dependent on social morale. Granting the fact that the greater number of Japanese made social organization easier, we still are at a loss to find a sufficient explanation of the dearth of outmarriages, unless we also concede the view that these people are better disciplined through being more conscious of social pressure and having the will to obey.

In all other groups, especially that of the whites, Adams fully recognizes the strong urge to find mates outside their own group, if women of their own race are not available. "Public opinion," he states (p. 53), "never develops among men isolated from women of their own race." Again he remarks: "A man is so fundamentally a social being that it is much more important for him to enjoy something approaching normal human relationships than it is that he shall have his first choice as to the character of the people with whom he associates" (p. 123). To the reviewer, this seems like a polite sociological statement of a common saying in Northwest Australia: "Necessity is the mother of half-castes."

Though it is true, as Adams suggests, that brides of their own nationality were more easily obtained by the Japanese than by the Chinese, this advantage was, with regard to the great body of immigrants, more theoretical than real. By the end of the Nineteenth Century only 20% of the Japanese were married, while even by 1916 the Japanese sex ratio was more highly abnormal than the average for the general population. In other words, there was a fairly lengthy period during which Japanese women were not available as brides and yet in this period Japanese out-marriages were comparatively rare. Some other explanation than those usually advanced would seem to be needed to account for this extraordinary fact.

In the reviewer's opinion, social conformity is easy for some, difficult for others. Temperamentally, some people are submissive to discipline, willing to remain in step, and wary of adverse public opinion. All individuals are susceptible to cultural training, but some are more susceptible than others. The psychologist, I believe, is inclined to give more weight to the facts of individual and group differences than is the sociologist. Adams, however, does not neglect this factor and the reader will find in various chapters, but particularly in the chapter (XVIII) dealing with the character of the mixed bloods, considerable discussion of this topic.

For those who are interested in what is probably the most intensive, though circumscribed, racial contact and amalgamation that the world has known, the skillful observations of the sociologist on the spot will have extreme interest and value. Hawaii maintains on the rim of her most active volcano a laboratory under the charge of a renowned volcanologist. The sociologists at the University of Hawaii have a similar point of vantage on the very edge of this miniature melting pot.

Adams' observations are characterized by shrewdness, common sense, and scientific detachment. Except for some tendency towards repetition, the book is interesting reading and can be highly recommended to students of social change.

S. D. PORTEUS.

University of Hawaii.

SEABURY, D. Adventures in self-discovery. New York: McGraw-Hill, 1938. Pp. ix+324.

The professional psychologist will find nothing of importance here. Good precepts for living, much talk about the soul, the power of the unconscious, subliminal forces, cosmic energy, and something vaguely termed "neurosis"—all expressed in a style which is sometimes simple and clear but quite as often vague and mystical—do not make up a very impressive book. Where it is intelligible, it is commonplace; where it is not commonplace, it is obscure and verbose. The author has evidently read widely but uncritically. All forms of psychoanalysis (J. B. Rhine, Eddington, Millikan, and Bohr) appear to be accepted as equally reliable.

EDMUND S. CONKLIN.

Indiana University.

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NOTES AND NEWS

DR. WILLIAM McDOUGALL, since 1927 professor of psychology at Duke University and from 1920 to 1927 professor of psychology at Harvard University, previously reader in mental philosophy at the University of Oxford and a fellow of Corpus Christi College, died on November 28 at the age of sixty-seven years.—*Science*.

DR. HARRY R. DESILVA, lecturer in psychology at Harvard University, has been appointed research associate in psychology at Yale University. He will have charge of a program of Automobile Driver Research in the Institute of Human Relations. Research on drivers will be carried out by a staff in coöperation with neighboring motor vehicle departments. The work is made possible by a grant to Yale University from the recently established Esso Safety Foundation.—*Science*.

THE Sixteenth Annual Meeting of the American Orthopsychiatric Association, an organization for the study and treatment of behavior and its disorders, will be held at the Commodore Hotel, Lexington Avenue and 42nd Street, New York City, on February 23, 24, and 25, 1939. Communications relative to this meeting should be addressed to Dr. Norvelle C. LaMar, Secretary, 149 East 73rd Street, New York City.

THE Washington-Baltimore branch of the American Psychological Association held its first meeting of the year at the Catholic University of America in Washington, D. C., on November 9, in connection with the celebration of the fiftieth anniversary of the founding of that University. The following program was presented:

C. J. CONNALLY, Catholic University: "Physique and Psychoses."

J. P. FOLEY, JR., George Washington University: "Effect of Prolonged Inverted Retinal Stimulation upon Spatially Coördinated Behavior in the Rhesus Monkey."

T. V. MOORE, Catholic University: "Theory of Perception in the Light of Pathology and Experimental Psychology."

A CONFERENCE on the Educational Production of Motion Pictures, sponsored jointly by the College of Education, Ohio State University, the National Council of Teachers of English, and the Film Project of the American Council on Education, was held on the campus of the Ohio State University on November 22 and 23, 1938. The program included demonstrations, discussions of techniques, and talks on the application of films to the various fields of education.

THE Institute of Educational Research, Teachers College, Columbia University, has completed the Semantic Word Count. This study, which was carried out under the direction of Dr. Edward L. Thorndike, lists all the words of more than 1 meaning which occurred in a sample of 2,500,000 words. It gives an estimate of the number of times a particular meaning is likely to occur in 1000 occurrences of a word. It also notes the number of different types of material in which that meaning is to be found; that is, its range of occurrence.

The Semantic Word Count furthers the principle of economical learning which was inherent in the publication of Dr. Thorndike's first count, *The teachers word book.* That book lists the 20,000 most common words and has been used as a guide for those who write or edit school readers and other textbooks. Educational psychology has by now accepted the idea that the more common words are the most important for a child to know.

There would be no further problem if all English words had but 1 meaning. But since this is not so, it becomes equally important to know which meanings or concepts of a word are the most important to teach. The Semantic Count offers the first quantitative approach to this problem.

Aside from its importance in grading readers, the Count provides an opportunity for the editors of school dictionaries to improve their product. There has always been a twofold problem for school lexicographers: which meanings to include and in what order meanings should be placed. The Semantic Count offers the solution to both of these questions.

The Semantic Count will also provide a useful instrument of research in the fields of language, psychometry, and adult education, especially the teaching of English to adult foreigners. Much interest has been expressed in the project by educators in charge of the program of teaching English as a secondary language in such places as India, Egypt, and the United States Territories.

THE Inter-Society Color Council will hold its annual meeting February 23, 1939, in the auditorium of the Electric and Gas Association of New York, 480 Lexington Avenue, New York City. The morning session will deal with the business of the Council, which will include the reports of committees through which the activities of the Council are carried on. In the afternoon and evening the following programs of invited papers of general color interest will be offered:

2:00 P.M. TECHNICAL SESSION ON COLOR TOLERANCES

This meeting is sponsored jointly by the Inter-Society Color Council and the American Psychological Association.

FORREST LEE DIMMICK, Hobart College, Chairman

The Physics of Color Tolerances. DEANE B. JUDD, National Bureau of Standards. The physicist evaluates color differences operationally in a standard coördinate system; perceptibility of color difference is relatively unimportant in tolerance consideration.

The Psychophysics of Tolerances. EDWIN G. BORING, Harvard University. The fundamental significance of the psychophysical methods and techniques in the evaluation of small sensory differences.

The Ratio Method in the Review of the Munsell Colors. SIDNEY M. NEWHALL, Johns Hopkins University. A promising application in a different field of a psychological method which is being used in color tolerance determination.

Color Tolerances as Affected by Changes in Composition and Intensity of Illumination and Reflectance of Background. HARRY HELSON, Bryn Mawr College. Typical data illustrating lawful relationships between the hue, saturation, and lightness of surface colors and principal conditions of viewing.

Representation of Color Tolerances on the Chromaticity Diagram. DAVID L. MACADAM, Eastman Kodak Company. The ICI coördinate system is recommended for color tolerance representations; use of a coördinate system based on just noticeable color-difference data would be unjustified and misleading.

Specification of Color Tolerances at the National Bureau of Standards. DEANE B. JUDD, National Bureau of Standards. Color tolerances have been applied by (1) specification of permissible area on a mixture diagram, (2) use of a standard and tolerance sample, and (3) use of the 'NBS unit of surface-color difference.'

Industrial Color Tolerances. ISAY BALINKIN, Cambridge Tile Manufacturing Company, Cincinnati, Ohio. Techniques and results in the establishment of color tolerances for particular products and purposes, and with special reference to ceramics. Control of color uniformity by determination of rates of color variation as a function of various physical or chemical factors.

COLOR TOLERANCE EXHIBIT

There will be an exhibit of color tolerance problems in connection with the afternoon program.

8:00 P.M. POPULAR SESSION: PARADE OF COLOR

M. REA PAUL, National Lead Company, Chairman

Each member body has been invited to contribute a short demonstration of recent color developments or of a research project of outstanding importance to its field. The purpose is to illustrate the color interests of each one of the member groups.

Demonstrations are assured of color in medicine; color in psychology; color in textiles; color in paper; color in lighting; and color in fashion.

Every effort will be made to present each story as dramatically as possible. It is appreciated that the evening session will attract many whose color experience encompasses only one limited sphere, and that their primary interest will be to understand how color is used in other fields. By this method a mutual appreciation of the future possibilities in coördinated color work will be engendered.

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Psychological Bulletin

THE ELECTRICAL RESPONSES OF THE EAR

BY ERNEST GLEN WEVER

Princeton University

The recent enlargement of experimental resources and techniques, and particularly the development of vacuum-tube apparatus for the amplification and recording of electrical potentials, has resulted in a broad extension of the field of sensory research. This review deals with recent work in which electrical methods have been employed in the investigation of the ear.

Electrical potentials as a result of stimulation of the ear by sound may be observed at various levels of the acoustic system: in the cochlea, in the auditory nerve, and in successive tracts of the central nervous system. It is the part of auditory research in this field to trace out the whole chain of events initiated by the entrance of sound waves into the ear and terminated by auditory perception and response.

A comprehensive review of this field has not previously been made, though there have been a number of treatments of restricted scope. Discussions of a limited character have been made by Caussé (**14**), Davis (**28**), Hallpike (**73**), Piéron (**117**), Reboul (**126**), Ruckmick (**128**), and Stevens and Davis (**135**). There have also appeared several summaries of the work of particular laboratories (Crowe and others, **18**, **90**, **91**; Davis and others, **29**, **36**, **45**, **134**, **135**) as well as discussions of the character and theoretical implications of the phenomena (Hallpike and Rawdon-Smith, **77**, **79**; Kreezer, **104**, **105**; Wever, **141**, **143**; Wever and Bray, **146**, **156**).

I. History

The history of this subject begins with early demonstrations of the electrical activity of the brain. Here Caton (**13**) was the pioneer not only in noting the presence of such activity in the cerebral cortex of animals, but also in showing that the activity is altered by sensory stimulation. He seems to have used visual stimuli only, and it remained for Beck (**9**) and Danilewsky (**27**) to show, several years later, that sounds are likewise effective in modifying the activity of the cortex. These studies, like the brain potential studies of more recent times, were concerned with the problem of cerebral activity, and not specifically with the working of the sensory system. They showed that sense-organ activity is reflected in electrical potentials, but their relation to further developments is incidental.

Piper (**118–120**) in 1906 made the first deliberate attempt to study the operation of the acoustic system by electrical methods. His problem was the function of the labyrinth in the fish, and he sought a solution by placing electrodes on the exposed otolith of the saccule and leading the currents to a galvanometer. Deflections were observed on stimulation under water with pipes of 100 and 260 vibrations per second and on striking the tank containing the preparation.

The first study of the electrical responses of the ear in mammals was that of Buytendijk (**12**) in 1910. He exposed the auditory nerve in rabbits and guinea pigs, applied electrodes which were led to a galvanometer, and observed deflections on stimulation with intense sounds of various kinds, such as a pistol shot and the note of a flute.

Forbes, Miller, and O'Connor (**54**) in 1927 looked to electric responses for an answer to the problem of the representation of frequency in the action of the auditory system. They placed electrodes on the brain stem or medulla of decerebrate cats, and with a string galvanometer recorded the impulses resulting from sound stimulation. Sharp noises, like the clicks of a watchman's rattle, gave impulses that were coincident with the stimuli. Continuous sounds, like the tone of a tuning fork, however, gave unsatisfactory results. A 104∼-fork produced responses that appeared 'imperfectly' synchronized with the sound waves, and a 200∼-fork seemed sometimes to do likewise. A card held against a cogwheel gave impulses up to about 220 per second. The authors concluded that the synchronization of nerve impulses and sound waves was more apparent than real, and was actually due to a piling up of impulses at the points of maximum amplitude of the waves.

In 1930 Foà and Peroni (**53**) worked on the giant sea turtle, and with a string galvanometer recorded impulses from the facial-auditory nerve (which is a single trunk in this animal) during acoustic stimulation. They observed discharges of 50–60 per second, irrespective of the character of the stimulus. In a later experiment on a different species of turtle, Wever and Bray (**148**) were unable to reproduce their results, and it is probable that the impulses were facial rather than auditory.

In all of the early studies there were severe limitations of method. The direct-current galvanometer is capable of revealing only slow alterations of potential. The string galvanometer records more rapid changes, especially if the string tension is raised, as in the experiment of Forbes, Miller, and O'Connor, and the resulting loss of sensitivity is compensated for by amplification. However, under the best conditions the string galvanometer is suitable only for low-frequency responses; in the experiment of Forbes, Miller, and O'Connor it became practically inoperative a little above 200~, though a somewhat higher limit is possible (**125**).

The development of vacuum-tube amplifiers for the faithful amplification of minute potentials has brought about a recent wave of activity in the study of the relations of the auditory system to its stimuli. In 1930 Wever and Bray (**144–147**) reported experiments in which an electrode was placed on the auditory nerve of the cat, with a grounded electrode elsewhere on the animal, and the currents led through a vacuum-tube amplifier to a telephone receiver. Responses were observed which were synchronized with the waves of sound over a wide range of frequencies. Extensive checks were made to exclude artifacts, and the phenomena were shown to depend upon the physiological activity of the auditory system.

These results have since been verified and extended by numerous investigators. The potentials generated in the cochlea have been distinguished and, in some degree, separated from those of the nerve itself, and limited observation has been made of responses at higher levels of the nervous system.

II. APPARATUS AND TECHNIQUE

Apparatus

The apparatus needed for experimentation in this field includes sources of sound, amplifiers, and recording equipment. The particular arrangements vary with problems and methods; some of these have been described by Wever and Bray (**147**, **155**), Witting (**167**), Garceau and Davis (**60**), and Hughson, Thompson, and Witting (**93**).

1. *Sources of Sound.* For exploratory study, various simple sources of sound have been employed, but exact work requires an audio-oscillator operating into a loud-speaker. This apparatus should have a wide range of frequency and intensity, and should

include suitable attenuators and filters for control of intensity and wave form. A determination of the absolute intensity of the sound stimuli is highly desirable, and may be made with a calibrated microphone.

2. *Amplifiers.* The gain and other characteristics of the amplifier will depend upon the method and equipment used in recording. Generally speaking, the amplifier should have the greatest gain and fidelity possible, and a wide range of frequency and intensity together with a minimum of background noise.

3. *Recording Apparatus.* The most generally satisfactory instrument for recording is the cathode-ray oscillograph. It has no troublesome frequency characteristics and withstands overload without harm. Its single drawback, low sensitivity, can be compensated for by high amplification. Observations can be made visually or photographically, and may include wave form as well as amplitude. Other measuring instruments, like the vacuum-tube voltmeter, give no indication of wave form, except indirectly when incorporated with additional apparatus for harmonic analysis.

Technical Procedures

1. *Anesthesia.* All the common anesthetics have been used, and none has shown any marked effect upon the cochlear response (**12**, **73**, **130**). Deep anesthesia, as from ether and chloroform, reduces the nerve potentials, especially those from higher tracts (**129**). Decerebration, with time for recovery from anesthesia and shock, gives most satisfactory nerve potentials.

2. *Operative Procedures.* The modes of surgical treatment vary according to the problem, but 2 approaches have proved particularly useful. These are the eighth-nerve approach and the middle-ear approach, respectively described for the cat by Wever and Bray (**147**) and by Crowe and Hughson (**18**, **90**).

3. *Electrodes.* The active electrode varies greatly in type and location. For the recording of impulses from the round window, cotton-wick and silver-foil electrodes are especially valuable. For recording from the nerve, a fine wire insulated except at the tip and the Adrian-Bronk hypodermic-needle electrode have been extensively employed (**130**). In the last type, the outer casing is commonly used as the grounded electrode; in the other types, a silver plate or other large conductor is placed in contact with some inactive tissue of head or neck. Nonpolarizable electrodes have been tried, but have no obvious advantages.

4. *Methods of Observation.* The methods of observation largely depend upon the recording equipment. In the early studies of Wever and Bray the amplifier was operated into a telephone receiver, and the method was one of simple listening. Nearly all investigators have followed this procedure for exploratory work.

The listening method has obvious characteristics that arise from the incorporation of the observer's ear in the system. It is arduous, slow, and inaccurate; yet not all of its characteristics are undesirable: high sensitivity can be achieved within the range of frequency in which the human ear is acute, and the analyzing power of the ear is an asset in the presence of a large background of extraneous potentials. For rough quantitative measurements the method has taken the following forms: (a) determination of the least intensity of stimulus to give a just-perceptible response; (b) determination, for a constant stimulus, of the amount of attenuation necessary to give a just-perceptible response; and (c) evaluation of the strength of the response by equating it to some external standard. Methods (a) and (b) are equivalent only if the responses are a linear function of stimulus intensity, which usually is not the case. Method (c) is likely to yield results that are inconsistent and misleading if strong stimuli are used and the responses fall in the region of overloading (160).

With oscillographs and meters a visual mode of observation is substituted for the auditory, and provides the advantages of ease, rapidity, and precision. For general exploration the auditory methods have their uses, but for quantitative work and studies in which the magnitude of responses is changing rapidly the visual methods are obviously preferable.

5. *Precautions.* In this type of experiment, many precautions are necessary to avoid the introduction of artifacts. The primary dangers are electrical induction and microphonics, which may be avoided by suitable electrical and acoustic shielding (147). The efficacy of these measures may be checked by the use of artificial preparations, tissue-killing reagents, and the like.

It may be mentioned that 2 electrodes in a vessel of water or salt solution, or the more elaborate device of Gatty and Rawdon-Smith (61) made of frog's skin in a tube of salt solution, will yield electrical potentials when exposed to intense sounds (see also Kupfer, 107). Likewise, potentials are generated by moist tissues anywhere on the animal. The distinction of these spurious effects from auditory responses is made initially in terms of their low magnitudes in relation to the magnitudes of the stimulus, and is further clarified by their functional relations to the stimulus and their reactions to anesthesia, drugs, blood supply, death, location of electrodes, and surgical procedures (**3**, **77**, **147**).

It cannot be too strongly emphasized that in this type of investigation great care and vigilance are necessary to guard against the entrance of error. There is little doubt that some of the discrepancies of experimental results which appear to perplex us are due to inadequate precautions.

III. GENERAL CHARACTERISTICS

The early work in this field revealed the physiological nature of the responses, and brought out the distinctive characteristics of the effects from the cochlea and from various elements of the acoustic nervous system.

The physiological nature of the responses was shown in a number of experiments (Wever and Bray, **147**). The responses decrease in intensity and finally fail on restriction of the blood supply and on death of the animal. A direct current applied to the tissues reversibly reduces or eliminates the responses, either through polarization effects or electrolysis. The responses are dependent upon the integrity of the auditory system, and disappear when the cochlea is destroyed or when the electrodes are shifted to other tissues remote from the cochlea.

The responses of the cochlea and of the auditory nerve differ in a number of respects, as has been brought out especially in the work of Adrian (**1**, **2**), of Adrian, Bronk, and Phillips (**3**), and of Davis and Saul (**40**, **129–131**).

1. *Localization.* The cochlear response has its focus of intensity at the cochlea itself and is most effectively picked up with the electrode on the round-window membrane. The nerve response is obtained from the nerve trunk, and, under conditions of low intensity of stimulation and moderate amplification, a movement of the electrode barely off contact with the trunk causes a significant drop or a loss of response. The responses in the central nervous tissues are restricted to the acoustic tracts.

2. *Anesthesia.* General anesthesia seriously affects the potentials from the central nervous system, but does not appreciably impair the cochlear responses.

3. *Blood Supply.* The nervous responses fail more rapidly than the cochlear responses after interruption of the blood supply to the head and after the death of the animal.

4. *Wave Form.* The cochlear responses reproduce the wave form of the stimulus with considerable fidelity. The nervous responses do so less exactly.

5. *Magnitude.* Under some conditions the nervous responses are stronger than the cochlear potentials. At high levels of stimulation, however, the cochlear responses are the stronger. The nerve responses have thresholds, while there is no indication of them for the cochlear potentials (**161**, **165**).

6. *Frequency Limits.* Cochlear responses may be observed to tones over a great range of frequencies, which varies with the species of animal studied (see Section IX). Nervous responses fall off rapidly in intensity as the frequency is raised, and under most conditions of observation they show a limit much below that of the cochlear response.

The above features distinguish 2 types of response, one in the sense organ, and the other at various levels of the acoustic nervous system. The cochlear responses persist after interruption of the central nervous connections, provided that the cochlear blood supply is maintained (**69**). On the other hand, the nervous responses are dependent upon the cochlea and cease after its destruction (**145**).

The relatively rapid disappearance of the nerve responses after interruption of the blood supply is probably not due to asphyxia of the nerve, but is an indirect effect which arises from the dependence of nervous upon cochlear activity. As the end-organ is impaired, both cochlear and nervous potentials decline, but the latter, which under most conditions of recording are relatively weak, are the first to disappear.

IV. THE COCHLEAR RESPONSE

The characteristics of the cochlear response vary somewhat with the species of animal concerned. The following discussion will deal principally with this response in the cat, an animal which has been extensively studied and whose ear represents a relatively high degree of development.

1. *Relation to Intensity.* For a tone of a given frequency, the cochlear response changes in a definite manner as a function of intensity. The form of the function has been studied by several investigators. According to the results of Wever and Bray (**155**, **161**), the most general form is that of a power function, in which the electrical response is proportional to the sound pressure raised to a power that usually is a little below unity. Typically, high tones yield

functions of somewhat smaller slope than intermediate tones, but individual differences are common. Covell and Black (**16**) obtained functions in which the slope (*i.e.* the power of the function) was unity, or nearly so. Davis and his collaborators have described the function variously, but in their most recent report (**135**) they stated that the 'ideal' form is linear, but that this form is usually departed from at low intensities. Wever and Bray (**161**) have presented evidence in support of their position that departures from a simple function at low intensities are probably due to extraneous potentials.

At high intensities the responses depart from the form described, as overloading appears (**95**). The curves approach a maximum and bend downward as the stimulus intensity is raised. A further increase in stimulus intensity greatly diminishes the responses, until ultimately the ear is damaged.

If the cochlear response is taken as indicative of the essential functioning of the receptor, as most investigators now agree, the form of the intensity function reveals that within its normal range of operation the ear is a sound transducer of great fidelity and efficiency. For intensities of great magnitude the efficiency falls, at first slowly, and then very rapidly. The form of the function at high intensities evidently means that central elements of the acoustic system are somewhat protected from excessive stimulation.

The maximum value of the cochlear response varies in a systematic manner for different frequencies. For the cat, the highest values of the maximum have been obtained for tones of the middle range (**16**, **161**). However, low tones have not been thoroughly studied in the cat. In the guinea pig they give very large potentials (**165**).

Under normal conditions, the intensity functions manifest high constancy: repeated measurements agree. This fact has led some investigators to use the maximum of the curves as a point of reference. Thus Davis and his associates (**36**) have often expressed their results in per cent of the maximum for a particular stimulus, and Covell and Black (**17**) have particularly recommended the use of the maximum for comparative purposes. However, it should be borne in mind that no single point on the intensity curve will serve as a substitute for the complete function. Responses at high levels sometimes behave differently in the face of adverse treatment from those at low levels. For example, in an experiment on the effects of the application of sodium chloride to the round window, Wever and Bray (**159**), under certain conditions, observed a reduction of responses that was progressively greater at higher levels of response: the intensity function suffered a general reduction in slope.

2. *Relation to Frequency.* Responses have been obtained to tones from 35~ up to 30,000~ (**158**). Cats have not been tested below 35~, but guinea pigs give responses to sinusoidal waves as low as 5~ (**165**).

A curve which expresses for a number of frequencies the sound intensity required to produce a response of a given magnitude is called an equal-response curve, and has been regarded as indicative (in an inverse manner) of the frequency sensitivity of the peripheral system. Equal-response curves vary in form for different animal species (**158**), and to a lesser degree for individual animals of a given species. They vary also with the level of response, by virtue of the fact mentioned above that the slopes of the intensity functions vary for different frequencies. At a relatively low level of response, a typical cat shows the highest sensitivity in the middle range of tones, from perhaps 500~ to 7000~, and the sensitivity falls off rapidly for lower and for higher tones. At high levels of response the curves for the cat usually show a particularly rapid loss of sensitivity for the lower tones (**161**).

A family of equal-response curves bears a resemblance to the curves of equal loudness for man. Stevens and Davis (**134**) have taken the position that "it is the behavior of the receptor mechanism which imposes the form which we find in equal loudness contours." Their conclusion was reached after a comparison of a selected loudness contour and what they termed 'thresholds' of cochlear responses for the guinea pig. Their 'thresholds' were minimum perceptible deflections on the cathode-ray oscillograph, and it has been pointed out (Wever and Bray, **161**) that their observations were as much a function of their apparatus and conditions as of the ear of the animal, and that the results for different frequencies were not comparable. Moreover, cats and other animals give different types of equal-response curves (**158**).

There is no question that if the cochlear response reflects the actions of the receptor its functions are ultimately related to those of loudness, but the intervention of central processes between the receptor and perception reduces the likelihood that this relation is a simple one.

3. *Wave Form.* For sounds of moderate intensity, the wave form of the cochlear response reproduces that of the stimulus (**1**, **125**, **144**). This relation probably holds generally, though observations at the lower frequencies of stimulation are complicated by the admixture of nerve impulses (**28**, **34**). At extremely low frequencies the nerve spikes become of relatively short duration in the cycle, and

thus are easily distinguished (**165**). In the guinea pig, reasonably good reproductions of the wave form of the stimulus continue as low as 15~ in some individuals, provided the intensity is only moderate. Lower tones give responses which are greatly distorted for all intensities at which they have been observed.

The responses to all tones show distortion at high levels of intensity, and depart from a sinusoidal wave form (**137**, **162**). An analysis of the waves reveals the entrance of numerous harmonics, often as high as the sixteenth for a 1000~ stimulus. At relatively low levels of response the partials have magnitudes which fall off in numerical order, but generally the odd partials have steeper slopes and at high levels outstrip their neighboring even partials. Thus, for 1000~ the third partial has a higher maximum than the second, and the fifth a higher one than the fourth. Sometimes a similar relation can be made out for the sixth and seventh and higher partials, but more often the magnitude of these is so low that comparison is difficult.

The increase in harmonic content with increase in intensity follows fairly closely the form of the intensity function in its departure from uniformity of slope. In one experiment (**162**) the total harmonic content rose from 10% at the first marked deviation from uniform slope to over 30% at the maximum of the curve, and finally reached 50% as the curve bent downward. Yet stronger stimuli would doubtless have revealed more severe distortion, as indicated by the waves obtained for very low tones, though these have not been analyzed (**165**).

A study of the results of wave analysis shows that the change of slope in the intensity function represents a true loss of efficiency and is not simply a transformation of energy into higher frequencies, since a summation of the responses for all partials gives the same form of curve as the fundamental alone (**162**).

If 2 tones are simultaneously presented, each produces its series of harmonics, and in addition there appear numerous interaction products in the form of combination tones. Newman, Stevens, and Davis (**116**) observed as many as 66 tones as the result of stimulating with 700 and 1200~ at high intensity. Also, the introduction of a new tone modifies the harmonic pattern of a tone already present.

The results on distortion indicate that the ear of the cat is both nonlinear and asymmetrical in its response to impressed vibratory forces. The mathematical expression of its action involves a

Fourier's series in which the necessary number of terms increases rapidly as the stimulus intensity is raised.

Stevens and Newman (**137**) cited evidence which led them to the conclusion that the muscles of the middle ear play a major rôle in the production of distortion. Deep anesthesia, which they believed to result in depression of tonus of the tensor tympani muscle, greatly increased the even harmonics, but failed to affect the odd. The results of Wever and Bray (**162**) do not agree with the conclusions regarding the action of the tensor tympani muscle. Denervation of this muscle and cutting its tendon produced no striking alterations in the pattern of distortion. Elicitation of the tensor reflex by stimulation of the pinna and the application of artificial tension to the tensor tendon produced a reduction of all components, with no characteristic difference in the effects upon odd and even partials. These results were interpreted to mean that tension of the tensor tympani muscle reduces transmission without otherwise seriously altering the mechanical state of the ossicular system. The behavior of the stapedius muscle in this relation is as yet obscure.

Distortion in the ears of animals as revealed by the cochlear response has been compared with distortion in man (**116**), and such a comparison substantiates the view that distortion is primarily, at least, a peripheral affair.

4. *Fatigue*. A depression of electrical responses as a result of stimulation of the ear was first shown in experiments of Davis and others (**35**, **36**) and of Hughson and others (**91**, **138**). A thorough investigation of the effect was made by Hughson and Witting (**95**). They found that after stimulation with a tone of extreme intensity the responses to all tones were impaired. Under their conditions the effect usually lasted for about 24 hours, and recovery occurred after about 72 hours. The effect was more marked when the muscles of the middle ear were divided. On the other hand, the effect was diminished or absent when artificial tension was exerted upon the middle-ear muscles, or when some other procedure reduced the efficiency of sound transmission. It was suggested, therefore, that the site of impairment was the sensory cells of the inner ear. There was no obvious damage to the organ of Corti, however (**93**).

Hughson and his associates worked with the electrode on the auditory nerve, and probably recorded both cochlear and nervous potentials. There is good evidence that both these types of response suffer 'fatigue' effects, but in the above results it is not possible to assign to each type its proper share.

Stevens and Davis (**134**) obtained essentially similar results from the round window, and their observations bear out the suggestion that the primary effects are located in the cochlea. They called the effect 'hysteresis.'

Wever, Bray, and Willey obtained impairments like those described only with low tones for which intensities up to 1000 dynes per sq. cm. were available (**165**). Other tones failed to show any reductions after many minutes of stimulation at intensities that gave maximal responses (unpublished observations). Hence it is likely that the other experimenters mentioned made use of intensities somewhat greater than those available to Wever, Bray, and Willey; it is unfortunate that they were unable to state the absolute intensities required for these impairments.

In view of the extreme intensities required, and the long period taken for recovery, it would seem more appropriate to regard the effect as one of injury rather than as fatigue in the usual sense. It is probable that stimulation deafness (see Section XII) is a later and irreversible stage of the same injury process.

V. Origin of the Cochlear Response

The physiological character of the cochlear response and its dependence upon the acoustic apparatus are unquestioned. Its specific point of origin within the cochlea and its relation to the process of nerve excitation, however, form matters of issue. Three theories regarding the origin of the potentials are: (1) the membrane theory, (2) the nerve theory, and (3) the hair-cell theory.

1. *The Membrane Theory.* Adrian (**2**) first suggested that the movement of fluids or membranes within the cochlea might give rise to the cochlear potentials. He gave up the hypothesis after he found a weakening of the responses as a result of cooling, and their failure on cocainization and death (Adrian, Bronk, and Phillips, **3**). Bast and Eyster (**7**) suggested a fluid pressure across some membrane as a possible source of the effects, and Leiri (**108**, **109**) discussed such a hypothesis.

Hallpike and Rawdon-Smith (**77**, **79**) have been the chief exponents of this type of theory. They suggested that the potentials are generated in the movements of fluids separated by a membrane, and pointed to the endolymph and perilymph, separated by Reissner's membrane, as being likely to fulfill the requirements. As these fluids are of different origin, they may be of different ionic concentrations, and thus may polarize the membrane.

2. *The Nerve Theory.* According to the theory suggested by Adrian, Bronk, and Phillips (3), and maintained for a time by Hallpike and Rawdon-Smith (71, 78), the cochlear potentials are generated by nervous elements within the cochlea. The latter adopted this view as a result of experiments on section of the eighth nerve in cats (78). In 3 animals, 6 months after a unilateral operation, tests showed absence of the cochlear response on the side of the section. In one of them, all parts of the ear appeared histologically normal except the neural elements, of which only a few remnants remained. These investigators later returned to the membrane theory after experiments by Guttman and Barrera (68–70) and further work of their own in collaboration with Ashcroft (6) revealed cases in which nerve section failed to impair the cochlear responses, though it caused degeneration of the neural elements. These later observations were verified by Hughson, Thompson, and Witting (94).

3. *The Hair-Cell Theory.* According to this theory, the cochlear potentials arise in the hair cells of the organ of Corti. The possibility of a sensory origin of these potentials was mentioned in some of the early discussions by Wever and Bray (145, 147), but the first specific indication of such an origin appeared in the work of Howe and Guild (83, 84) on albino cats. Electrical tests of these animals revealed the absence of cochlear potentials, and a histological examination showed practically complete absence of the organ of Corti and its hair cells, with collapse of the cochlear duct and shrinkage of the stria vascularis, but no other cochlear abnormalities. Similar observations were later made by Davis and his collaborators (36, 110, 114) on both albino cats and waltzing guinea pigs, and by Hughson, Thompson, and Witting (93) on congenitally deaf Dalmatian dogs.

The hair-cell theory has been most explicitly and energetically supported by Davis and his associates (28, 36, 135). They reported in their studies of stimulation deafness and of cochlear destruction a close correlation between losses of cochlear response and losses of auditory acuity as shown by conditioned response tests (33). Fowler and Forbes (56, 57) likewise accepted the hair-cell theory on the basis of their studies of the effects of chemical substances upon the inner ear.

In recent discussions, Hallpike and Rawdon-Smith (79) have reviewed the evidence on the above 3 theories and have concluded in favor of the membrane theory, while Stevens and Davis (135) have made an able defense of the hair-cell theory. The nerve theory as a complete explanation seems now to have no adherents, but it is generally agreed that a small amount of nerve potentials may be recorded from the cochlea as a ' contamination ' of the cochlear potentials proper. This fact was first pointed out by Davis, Derbyshire, and Lurie (**34**) and has since been confirmed (**45**, **165**).

The hair-cell theory rests primarily upon the evidence from albino animals. Hallpike and Rawdon-Smith argued that this evidence is inconclusive, since in addition to hair-cell atrophy there was often collapse of the cochlear duct and a displacement of Reissner's membrane and the tectorial membrane from the usual positions. They did not accept the evidence of Davis and others (**33**) on stimulation deafness, for they maintained that the widespread damage which is typical of such experiments does not permit any reliable correlations of loss of response and hair-cell atrophy.

As more direct evidence for the membrane theory, Hallpike and Rawdon-Smith cited the experiment of Gatty and Rawdon-Smith (**61**) with a model consisting of a membrane of frog's skin bounded by fluids, and observations on the effect of eighth nerve section (**6**, **79**). After nerve section, they observed cases in which cochlear responses were absent though the organ of Corti with its hair cells was morphologically normal, and other cases in which cochlear responses were present but the organ of Corti and its hair cells were grossly degenerate.

A further argument for the membrane theory, as against the hair-cell theory, is based upon recent experiments by Hallpike, Hartridge, and Rawdon-Smith (**75**). They studied the effects of a sudden change of the phase of a stimulating tone upon responses from the cochlea and brain stem. The cochlear responses were found to follow the phase change with considerable exactness. The nerve responses, on the other hand, usually showed a marked reduction in amplitude for a short interval after the phase change. The argument is that the ' silent interval ' in the nerve response reveals the presence of some resonant structure, probably the basilar membrane, in the path between stimulus and nerve action, whereas the absence of any such ' silent interval ' in the cochlear response places this response outside the resonant structures, and thus most likely in the movement of some membrane.

A different form of membrane theory is held by Eyster, Bast, and Krasno (**50**), in which it is suggested that the electrical effects arise as ' streaming potentials ' in the flow of liquid through a porous membrane (cf. Leiri, **109**). From various experiments, including the application of chemical substances to the round window, the passage of direct current through the cochlea, and exploration of pathological conditions of the inner ear, in all of which a histological examination followed the study of cochlear potentials, they concluded that "the development of cochlear potentials . . . does not depend on the structural integrity of the organs of Corti or associated structures."

It is far from easy to evaluate the evidence and arguments cited above, and at present a decision is largely a matter of personal judgment. There is no doubt that movements of fluids and membranes can generate electrical potentials, but in the writer's opinion such potentials in the cochlea are of negligible magnitude. The argument from the experiments on phase reversal includes 2 assumptions that may be questioned. The first assumption is that the basilar membrane is a resonant structure with damping so low that the membrane could not follow a change of phase. There is abundant evidence that damping in the ear is of high degree. The second assumption is that the 'silent period' in the brain-stem response is due to resonance in the basilar membrane or elsewhere in the end-organ. Other explanations are possible. The effects might be accounted for as a momentary inhibition of the usual impulses by other impulses generated by transients. That transients were present, and probably arose within the ear, is suggested by the form of the cochlear potentials as portrayed in sample curves, for this form appears to differ significantly from that of the stimulus as recorded with a microphone. The observations of Eyster, Bast, and Krasno are not in accord with those of most other investigators in this field. In the writer's opinion, the weight of the evidence is in favor of the view that the cochlear potentials are generated by the hair cells.

VI. AUDITORY NERVE RESPONSES

The responses of the auditory nerve may be observed at the nerve trunk or, under some conditions, at the round window. Both types of observation are attended with technical difficulties due to the presence of cochlear potentials. At the nerve trunk the cochlear potentials can be minimized by the use of a small electrode placed as far from the petrous bone as feasible and by the use of moderate stimulation and amplification. A shielded electrode of the Adrian-Bronk type is suitable, though the efficacy of the shield as such in excluding extraneous potentials is open to question.

When both auditory and cochlear potentials are present in the response, as often happens in spite of precautions, they nevertheless may be distinguished by particular features. A number of these, such as susceptibility to drugs, anesthesia, cold, death, and polarization, have already been mentioned. Derbyshire and Davis (**45**) regarded 2 tests as particularly useful: (1) the longer latency of the nerve response, and (2) the relation of its polarity to the stimulating wave. According to their measurements, the latency of the cochlear response is very small, 0.1 millisecond (ms.) or less, whereas that of the nerve response is 0.5 ms. or greater. They have further reported that the polarity of the cochlear response changes with that of the sound waves, while the polarity of the nerve response remains constant.

Davis and others (**35**) described an 'on-effect' for the nerve response, a large, momentary burst of impulses at the beginning of stimulation. This effect was said to be especially prominent at higher levels of the nervous system (**28**). A somewhat different 'on-effect,' as well as an 'off-effect,' was found in the cochlear response, and was supposed to reflect the resonance characteristics of the peripheral system. However, Snellen (**133**) was unable to observe such effects when precautions were taken to exclude clicks at the time of turning the stimulus on or off.

The nerve response is subject to equilibration; an initial large response falls off, at first rapidly, and then more slowly, until equilibrium is reached after about 7 minutes of steady maximal stimulation. The amount of decline of the response is said to vary systematically with frequency, and to be greatest around 1000, 2000, and 3000~. This observation, together with that of variations of initial amplitude for different frequencies, led Derbyshire and Davis to the conclusion that as the stimulating frequency is raised the nerve fibers go into 'alternation' at critical points. From observations of critical points in the frequency scale, Derbyshire and Davis (**45**) inferred that the 'functional refractory phase' of the auditory nerve is about 0.1 ms. This 'alternation' of activity is the same assumption made earlier by Wever and Bray (**146**) under the volley hypothesis in explanation of the high rates of impulses which they observed in the nerve. This hypothesis states that when the stimulating frequency becomes so great that the refractory period exceeds the period of the sound waves, a nerve fiber responds at every second wave, or every third, and so on. If different nerve fibers come into action for different waves, the total response can continue to represent the stimulus frequency. However, the appearance of 'critical points' is unexpected, for it is difficult to see how the fibers of the nerve can have refractory properties so similar, and be excited so uniformly, as to give this result. The observation of critical points thus needs confirmation.

The nervous discharge is synchronized with the sound stimulus for all low and intermediate frequencies. The lower limit extends at least to 15~, and perhaps would extend farther were it not that below this point sensitivity falls off with great rapidity, and at the high stimulus intensities necessary the distortion is so serious that harmonic frequencies dominate the responses (Wever, Bray, and Willey, **165**). The upper limit of synchronism evidently depends in part upon conditions of observation, for various figures have been given. According to Derbyshire and Davis (**45**) this limit is between 3000 and 4000~.

Asynchronous responses continue to higher limits. In the opossum McCrady, Wever, and Bray (**115**) obtained responses from the eighth nerve up to 11,000~ in one animal and 15,000~ in another.

Early observations by Wever and Bray (**141**) and by Saul and Davis (**130**) indicated for the nervous response a different relation to intensity from that for the cochlear response. The nervous response was described as rising relatively slowly as stimulus intensity was increased. Wever and Bray suggested that the Weber-Fechner relationship might be found between cochlear and nervous responses. More recently, however, Derbyshire and Davis (**45**) described the function of the nerve response as similar to that of the cochlear response.

This problem of the functional relation of the nerve response to the cochlear response is complicated by irregularities in the form of the nerve potentials. Observation of these potentials at the round window shows that for very low tones an increase of intensity gives first an increase of amplitude and then a broader wave of response. Evidently at high levels the nerve impulses contributing to the response become more spread out in time (**165**).

For the nerve response, little is known as yet of the variation of sensitivity with frequency of stimulation. Observations of 'thresholds' by Derbyshire and Davis (**45**) gave results similar in form to those for the 'thresholds' of the cochlear response, but it is likely that the resemblance is more a function of the conditions of observation than of the phenomena themselves (**161**). The results vary with the location of the electrode in the nerve, an observation which has been interpreted to mean that particular fiber tracts transmit impulses for particular tones (**45**).

Derbyshire and Davis made an extensive study of the nerve responses to clicks. Such stimuli produce multiple potentials, usually 3 in number, which are distinguished by their latencies. The latency of the first of these potentials was said to bear a constant relation to the negative peak of the cochlear response. Hissing sounds and tones produce masking effects that vary for the different potentials according to their latencies and also according to the frequency of the masking sounds. These observations on masking were regarded as additional evidence that different fibers transmit impulses for different frequencies (**45**).

Nerve Excitation. Three hypotheses have been advanced to explain the process of nerve excitation. These respectively regard the process as mechanical, electrical, or chemical.

According to the mechanical hypothesis, it is assumed that changes of pressure are communicated to the nerve terminations at the hair cells and excite these terminations directly. It follows that the cochlear potential, arising in the deformation of the hair cell, is incidental to the excitatory process.

According to the electrical hypothesis, the cochlear potential itself excites the nerve terminations.

According to the chemical hypothesis, a deformation of the hair cells liberates a chemical substance which acts as a mediator in the excitation of nerve fibers.

Davis and others (**36**) for a time accepted the electrical hypothesis, but later, in consideration of the relatively long latencies of nerve responses, gave it up in preference to the chemical mediator theory (**44**, **45**). It is not clear, however, how the chemical mediator theory can account for time relations of excitation so precise as to give synchronous nerve volleys as rapid as 3500 or more per second.

VII. RESPONSES OF THE CENTRAL NERVOUS SYSTEM

A number of experiments have studied electrical responses from the acoustic tracts of the central nervous system. These responses have been observed at the cochlear nucleus, traced through the trapezoid body and lateral lemniscus to the inferior colliculus, thence through the inferior quadrigeminal brachium to the medial geniculate body, and finally by way of the acoustic radiations to the temporal cortex.

The effects are found alike in homolateral and contralateral pathways (**145**, **147**). Saul and Davis (**129**) reported that the responses were greater in the contralateral tracts, but at the lateral lemniscus Kemp and Robinson (**102**) found homolateral and contralateral effects to be equal. In the various acoustic tracts the responses are highly localized: usually only a slight movement of the electrode causes a great change in amplitude (**40**, **130**, **147**). At some points, perhaps due to the presence of cell-bodies, the potentials exceed those recorded from the nerve trunk (**129**).

The central effects are highly susceptible to such adverse conditions as drugs, anesthesia, fatigue, circulatory deficiency, and death. Their latencies are relatively long, and increase at higher levels of the system, due to the increase in length of pathway and the addition of synaptic junctions. The time of synaptic delay was calculated by Kemp, Coppée, and Robinson (**98**, **101**) as in the region of 0.8 ms.

The relations to stimulus frequency vary with the level of the nervous system at which the observations are made. In general, the limits of frequency for which potentials may be obtained are below the limits shown by the nerve trunk, and decrease further as higher neural levels are reached. Kemp, Coppée, and Robinson (**101**) observed synchronous responses up to 2500~ at the trapezoid body, but only to 1000 or 1500~ at the inferior colliculus; for higher frequencies the responses were for a time partially synchronous, then wholly asynchronous, and finally absent.

At the trapezoid body the upper limit of frequencies for which asynchronous responses were found was 4000~, and at the inferior colliculus this limit was 1000~; higher tones gave only a momentary effect as the switch was opened (**100**). These results were obtained in the cat. McCrady, Wever, and Bray (**115**), working on the opossum, obtained responses from the trapezoid body up to 9000~. The difference is probably as much a function of recording apparatus as of animal species. Gerard, Marshall, and Saul (**63**) reported synchronous responses in the region of the corona radiata to a tone of 660~. At the cortex Davis (**28**, **29**) reported no synchronism to tones, though a succession of clicks was reproduced up to 100 per second.

Evidence for the specific action of different fiber groups for different stimulus frequencies was brought forward by Kemp and Coppée (**99**) for the inferior colliculus and by Gerard, Marshall, and Saul (**63**) for the cortex. Kemp and Coppée stimulated with clicks, and found the usual responses to be obliterated by tones whose necessary frequency varied systematically with electrode position.

The relation of the central responses to stimulus intensity was studied at the lateral lemniscus by Kemp, Coppée, and Robinson (**101**). They found the magnitude of the potentials to increase with intensity, sometimes in a regular and sometimes in a rather irregular manner, and finally to pass through a maximum. The function differed with the position of the electrode, and a new position often yielded responses greater than the maximum obtained from a previous location. Though these writers do not point it out, their more regular functions show through the greater part of their course a logarithmic relation between stimulus and electrical responses; as will be discussed later, this relation is important for the Weber-Fechner law. These investigators found that for low tones the sensitivity at the lateral lemniscus is greater than for cochlear responses at the round window.

Further studies by Kemp and Robinson (**102**) at the lateral lemniscus revealed a number of characteristics of binaural stimulation. For low intensities, stimulation of both ears equally gave amplitudes of response which represented a simple addition of effects. There was no appreciable difference in the latencies for homolateral and contralateral pathways. No binaural masking was observed; and if the 2 ears were stimulated at slightly different times there was no inhibitory effect, and the proper latencies were preserved. These results are of particular value for the problem of the binaural localization of sounds.

Much of the evidence on central nervous processes is fragmentary, and therefore difficult to evaluate. There is little question that the failure of agreement of many of the observations is partly due to differences in technique and in sensitivity of recording apparatus. Differences in sensitivity will affect particularly the limits of observable responses.

VIII. EMBRYOLOGICAL DEVELOPMENT OF THE RESPONSES

The electrical responses of the ear have been employed in the study of the development of auditory function in relation to the structural differentiation of the ear. This investigation has been carried out on the opossum, one of the few mammals in which the ear is immature at birth. McCrady, Wever, and Bray (**115**) observed the cochlear responses in various members of a single litter of pouch-young at intervals of 59 to 82 days after birth. The results showed responses in the youngest animals to tones of middle frequencies only (about 500 to 5000~), and in older animals a progressive extension of the range to include both lower and higher tones. At the same time, sensitivity increased greatly for all tones and especially for high tones. Maximal sensitivity was first shown in the region of 2000~, but with growth this maximum shifted upward and finally lay in the region of 7000~, where it is still found in the adult opossum. At 80 to 90 days of age the ear appeared to have reached its maturity. These results are confirmed by observations on a second litter (unpublished).

Studies of reflex movements in response to sounds in the above animals yielded results in general agreement with the electrical studies. Moreover, the growth of function correlates well with the structural differentiation of the organ of Corti, which begins in the distal position of the basal coil of the cochlea and thereafter extends in both directions.

IX. PHYLOGENETIC STUDIES

A number of species of animals have been investigated by the electrical method, with results that are pertinent to a consideration of hearing in relation to the phylogenetic series. The species studied include several insects, and one or more representatives of each of the classes of vertebrates.

Insects. Only a few species of insects possess an organ of a form and complexity such as to indicate a true auditory function. The most familiar of these, the grasshoppers, katydids, and crickets, are found in a single order, the Orthoptera. On the forelegs of katydids and crickets and on the first abdominal segment of grasshoppers is borne a special organ for sound reception, which consists of a tympanum in relation to specialized sensory cells (scolophores) and a sensory nerve. Electrical responses may be recorded from either the nerve or its central ganglion.

Crickets of the species *Gryllus assimilis* as studied by Wever and Bray (**151**) usually gave responses over the range of 300 to 8000~. Katydids of the species *Amblycorypha oblongifolia* showed a greater range for high frequencies; one specimen responded from 800 to 45,000~. Grasshoppers (*Arphia sulphurea*) responded from about 300~ to about 20,000~ (**142**). Measurements of absolute sensitivity in the grasshopper showed this ear to be very deficient in response to low tones, but to be as sensitive to high tones as the ear of man (**122, 142**).

The tympanal organs so far studied give only asynchronous potentials over the range of frequencies to which they respond. This may be true because the number of nerve fibers supplying these organs is small. At any rate, certain other sensory endings of insects, of a much simpler form but more richly supplied with nerve fibers, give synchronous potentials. The cercal apparatus of the cockroach (*Periplaneta americana*) and also of the cricket (*Gryllus domesticus*), which consists of sensory hairs and other endings supplied by the cercal nerve, was found by Pumphrey and Rawdon-Smith (**123**, **124**) to respond readily to sound waves of low and intermediate frequency, up to about 3000–4000~. At the lower end of this range the responses were often synchronized with the stimulus, but at times other relations, as frequency-doubling, frequency-halving, etc., were observed. These results are significant for their resemblance to the frequency relations of the electrical responses in animals with a much more elaborate acoustic apparatus.

Fishes. As there is good evidence for hearing in certain fishes, we should expect to obtain electrical responses from their ears. So far, however, the only positive results are the early observations of Piper (**118–120**), mentioned above. Wever and Bray have made attempts to record potentials in the goldfish, but with results which, so far, are uncertain.

Amphibians. In this class of vertebrates only the frog has been studied. Ashcroft and Hallpike (**4**, **5**) obtained responses from the saccular nerve when the platform bearing the preparation was jolted or caused to vibrate by applying the stem of a tuning fork. Synchronous responses were secured up to 512~, and momentary responses of uncertain synchronism at 1024~. Since air-conducted sounds were ineffective, Ashcroft and Hallpike concluded in favor of the view that the saccular organ of the frog and also of higher forms serves a distinct vibratory function: the reception of bone-conducted sounds. However, Wever and Bray (**154**), in experiments on the bullfrog (*Rana catesbiana*), observed responses which appeared readily to air-conducted sounds. The range of frequencies for which responses were obtained extended to 500 and sometimes 600~, a limit which agrees well with that observed by Ashcroft and Hallpike. In view of these results, the conclusion regarding a special vibratory function for the saccule must be regarded with some doubt.

Reptiles. As already referred to, Foà and Peroni (**53**) recorded from the facial-auditory nerve of the giant sea turtle (*Thalassochelys caretta*) and obtained responses around 50–60 per second, irrespective of the stimulus frequency. From the separate cochlear nerve of the common painted terrapin (*Chrysemys picta*) Wever and Bray (**148**) observed large synchronous responses up to 500~, and responses of diminished magnitude up to 1000 and sometimes 1200~. The sensitivity to low tones seemed somewhat greater than that found for mammals, but no precise measurements were made. The ear of the turtle appears to be superior to that of the frog in range, and possibly also in sensitivity.

Birds. Among birds, only the pigeon has been studied. Wever and Bray (**153**) recorded responses from the round window over a range of frequencies which usually extended from 100 to 10,000~, but was sometimes slightly wider. These limits are in close agreement with the limits of hearing obtained in conditioning experiments. The responses were synchronized with the stimulus frequency and reproduced its wave form. The relation to stimulus intensity is fairly regular, and resembles the functions obtained for mammals except for a characteristically lower slope. The slope is usually around 0.4, as compared with slopes near unity for mammals; this indicates a relatively low efficiency in sound transmission.

The sensitivity of the pigeon's ear as revealed by electrical responses is only moderately inferior to that of cats and guinea pigs at low intensities of stimulation, but at high intensities it becomes decidedly inferior. Sensitivity is greatest near 2000–3000~, and falls off particularly rapidly for higher tones.

Mammals. Studies of electrical responses among mammals include 8 species: opossum, rat, rabbit, guinea pig, cat, dog, monkey, and man. Of these, only the opossum, guinea pig, and cat have been extensively dealt with.

The opossum is of particular interest because it is one of the most primitive mammals. Cochlear responses are found for a considerable range from about 100 to 25,000~, and show a maximum of sensitivity around 7000~. Nerve responses have been observed up to 11,000 and 15,000~. These limits agree well with results for pinna reflexes and startle reflexes in response to tones; the maximum range over which such reflexes have been observed is 100 to 19,000~ (**115**).

A few observations have been made of nerve responses in rats and rabbits (**140**), but the quantitative data are very limited.

Cochlear responses have been observed in the guinea pig to tones from 5 to 25,000~ (**158**). At all but the lowest frequencies, the responses reproduce with reasonable faithfulness the wave form of the stimulus. Between 5 and 15~ the distortion is great, and the observed responses are made up of harmonics of the stimulating frequency (**165**). For tones of the middle range the sensitivity is relatively high, and reaches its maximum in the region of 600~ (**155**).

The cat has been used for electrical studies more extensively than any other animal. The limits of cochlear responses extend from 35~ or lower up to 30,000~. The sensitivity compares favorably with that of the guinea pig; in general the sensitivity of the cat is poorer than that of the guinea pig for low frequencies but better for high frequencies (158, 161).

Cochlear and nervous responses have been observed in both dogs (8, 11, 19, 25) and monkeys (62, 63), but no extensive quantitative data have been obtained.

Several attempts have been made to obtain cochlear responses in man. An early effort by Wever and Bray to record such responses from the exterior of the ear gave no results. Grossmann and Spielmann (65), Van Gilse (64), and others likewise have reported negative results. In one experiment, Fromm, Nylén, and Zotterman (58) used patients with perforations in the drum that were large enough to give access to the round window, but the same methods that yielded responses in the cat were unavailing. In a later series (59), with a different arrangement of amplifying apparatus, positive results were obtained from 2 patients—in one of these from both ears. In the best of these experiments observations were made over a range of 200 to 8192~. The responses were much weaker than responses obtained similarly from cats, but the difference was probably due at least in part to the fact that the patients were defective in hearing.

If a technique for human recording could be developed with a practical degree of sensitivity it would undoubtedly be of service in clinical diagnosis. It is disappointing that attempts so far have been largely barren of results. The failures seem unaccountable except on the basis of variations of technique or defective hearing in the subjects, for we know of no important structural differences between human ears and the ears of cats, guinea pigs, and monkeys, and thus have every reason to expect large cochlear potentials.

The results on various species of animals are hardly sufficient as yet to form a complete picture of the phylogenetic development of auditory function. The results on the insects, in conjunction with those from behavioral studies, are of interest in revealing the characteristics of a peculiarly isolated form of vibratory receptor. The evidence on the vertebrates, so far as it goes, indicates that within this subphylum the development of hearing runs a fairly direct course. From fish to mammal, it is marked by a reasonably consistent gain in frequency range. Whether there is also a gain in maximal sensitivity remains to be learned, but there is little doubt that as development has proceeded the region of greatest sensitivity has shifted in the direction of the higher frequencies.

In the various animals, the results from electrical studies are consistent with what we know of hearing on the basis of behavioral tests. Comparisons must be made with caution, since the results of the electrical studies are always limited to some extent by recording apparatus, and also because the animal's reactions to sound are determined by central and motor as well as by peripheral processes (**140**). However, the conditions permit a comparative judgment of the limitations which the peripheral mechanism imposes. In the animals adequately studied by both methods (cat, opossum, and pigeon), the limits of electrical responses bear to one another a relation similar to that shown in behavioral studies.

X. Studies of the Middle Ear

Crowe and Hughson (**19**, **88**) first pointed out the usefulness of the electrical method for a study of the function of particular parts of the ear, and the method has been applied by them and others in investigations of the drum, ossicles, tympanic muscles, middle-ear pressure, and infectious conditions of the middle ear.

1. *Ear Drum.* Early studies by Crowe and Hughson (**19, 88, 90**) revealed no appreciable change in responses after simple incision of the drum or the damping of limited areas with vaseline or pledgets of cotton. Covering the whole surface, however, impaired the responses, especially those for low tones. Recently Bordley and Hardy (**10**), with improved methods of observation, found that small but consistent impairments result from drum incisions. The losses vary with the site of the incision and its extent and are relatively greater for tones of low frequency and of high intensity. Their results agree closely with audiometric studies of the effects of simple traumatic rupture of the drum in man.

2. *Ossicles.* An interruption of the ossicular chain has a profound effect upon the electrical responses to air-conducted sounds (Crowe and Hughson, **19**), but affects the reception of bone-conducted sounds relatively little (Guild, **67**; Wever and Bray, **152**). The losses for air conduction are about 40–60 db., and are somewhat greater for high than for low tones. Any mechanical interference with the movements of the ossicles impairs the responses (**19**).

3. *Tympanic Muscles.* In cats and guinea pigs under light anesthesia, the muscles of the middle ear frequently show momentary contractions (Davis and others, **33**). Also, the contractions are

elicited by stimulating the pinna, the external meatus, and the facial nerve (Hughson and Crowe, **90**; Hughson and Witting, **95**). Hallpike (**72**) reported reflex contractions to sounds, but Hughson and his collaborators (**90**, **95**) were unable to confirm him. The muscle contractions, whether 'spontaneous' or elicited artificially, cause marked reductions in the magnitude of cochlear potentials. The amount of reduction may be made to vary according to the intensity of stimulation of the pinna (**162**).

The function of the tensor tympani muscle was investigated in the cat by Crowe, Hughson, and Witting (**19**, **21**, **90**) by artificially reproducing its action. Tension was exerted on the tendon by means of an attached thread and observations were made of the resulting reductions of response. Further results by a slightly different method were obtained by Wever and Bray (**160**). Under most conditions of stimulation, the amount of diminution of the responses bears a simple relation to the degree of tension; as tension is increased the responses fall, at first rapidly, and then progressively more slowly. If high intensities of stimulation are employed, so that the region of overloading is reached, the tension curves take various forms depending upon the degree of overloading. Of particular interest is the case in which the overloading is severe and the position on the intensity function is beyond the maximum; for such a stimulus the application of tension first causes an increase in the cochlear response, and finally, if sufficient tension is applied, is followed by the usual decrease. These observations suggest that overloading appears beyond the middle ear, possibly in the responses of the hair cells, and that tension, by reducing transmission, reduces the severity of overloading.

The effects of tension upon the responses varies with frequency of stimulation in a manner that suggests a relation to sensitivity (**160**).

Wiggers (**166**) studied the effects of 'spontaneous' contractions of the muscles upon the cochlear responses. As a result of contractions he observed considerable reductions of response for tones below 1000~, and the reductions were greater the lower the frequency. On the other hand, the transmission of tones between 1300 and 1800~ was enhanced, while transmission above 2000~ was unaffected. As this work was done on the guinea pig, there is no necessary disagreement with the results already mentioned for the cat. It is possible that the variation with frequency is related to the sensitivity of the animals, though the results are not sufficient to determine this point.

No immediate effect upon the cochlear responses has been observed as a result of denervation of the tensor tympani muscle, or cutting its tendon (**19**, **163**).

Relatively little information is available on the action of the stapedius muscle. Hughson and Witting (**95**) reported that tension upon its tendon produces more marked effects than the corresponding treatment of the tensor tympani muscle. They reported further that overloading develops earlier when the tendons of both muscles are divided.

Stevens, Davis, and Lurie (**136**) reported, in a single case, a great and evidently permanent loss of responses as a result of sound stimulation in an animal whose tensor tympani tendon had been divided. This observation has not been verified.

Culler and Finch (**24**), in a stimulation deafness experiment, obtained impairments which were especially pronounced when the tensor tympani tendon was cut. On the other hand, Hughson and Witting (**95**) observed the effects of detonations to be less when the tensor and stapedius tendons were divided than under normal conditions. There is no real discrepancy between these observations if we admit a twofold function for the tympanic muscles, one of protecting against excessive stimuli, the other of adding strength and rigidity to the ossicular system. In the presence of detonations the muscles give no protection because their latencies are so long that damage is done before they begin to act. Hence their absence is no handicap; rather, it is a help, for when the ossicles are in a more fragile condition they fail first and the sounds do not reach the inner ear to do more serious damage there. In the stimulation deafness experiments, in which the stimuli are continuous, the muscles are able to exert their protective action, and hence their absence is a disadvantage.

4. *Middle-Ear Pressure.* The relation of pressure in the middle-ear cavity to the electrical responses was studied by Thompson, Howe, and Hughson (**138**). They varied the pressure by means of an air pump connected to a needle fixed in the Eustachian tube, and recorded the resulting pressure with a manometer connected with a second needle cemented in the auditory bulla. The results showed impairments for both positive and negative pressures in amounts which increased with the departure from normal. The effects were greater for high and low tones than for tones of the middle range.

5. *Pathology.* Pathological conditions of the middle ear are occasionally encountered in animals, especially in cats and rats.

Such conditions usually cause a marked reduction in cochlear responses (**91**, **114**). Reductions as great as 40 to 60 db. have been reported as a result of middle-ear infections, particularly those in which the ossicles were seriously affected. Responses to low tones are impaired somewhat more than those to high tones, and overloading for strong stimuli usually fails to appear (**95**).

XI. STUDIES OF THE INNER EAR

1. *Round Window.* A number of efforts have been made to determine the function of the round window. Hughson and Crowe (**19**, **89**) exerted pressure on the round-window membrane with a pledget of cotton, and observed an improvement of responses. They then tried to obtain permanent effects by grafts of fascia, periosteum, and bone in the round-window niche, and reported significant improvements of response (**89**, **90**). These experiments were made on cats, but the suggestion was advanced that such grafts in human ears might serve for the relief of some kinds of deafness.

Finch, Culler, and Girden (**25**, **52**) repeated the above experiments, though with a somewhat different technique. A soft-gum plug was placed in the round-window niche of dogs, and tests were made by both electrical and conditioning methods. Both methods agreed in showing impairment as a result of the plug (**26**).

Hughson and Crowe had already pointed out that simple blocking of the round window with cement produced no change, and it is likely that the gum-plug experiment is more comparable to this procedure than that designed to produce positive pressure. Thus the degree of disparity of the experiments of Hughson and Crowe and those of Finch, Culler, and Girden is not yet clear. Recently, Hughson (**87**) has proceeded with graft experiments in human patients, with results which cannot yet be suitably assessed.

More drastic procedures, such as puncturing and tearing the round-window membrane, have yielded various results (**19**, **88**, **167**). Davis and others (**36**) reported impairments which in different trials ranged all the way from nearly none to 90%. It is likely that in these experiments there were large variations in the operative damage. Hughson and Crowe (**90**) pointed out that at times the manipulations caused fracture of the basal osseous lamina, and the serious losses no doubt are to be attributed to this cause.

2. *Intralabyrinthine Pressure.* Observations on the effects of pressure on the round-window membrane raised the problem of the relation of intralabyrinthine pressure to the action of the ear, and

cast doubt upon the general belief that an increase in such pressure interferes with hearing. Investigations by Hughson and Crowe (**85**, **90**) showed that electrical responses are impaired if intralabyrinthine pressure is sharply decreased, but are unchanged if pressure is increased. The method was the intravenous injection of hypertonic salt solution for reducing the pressure, and distilled water or hypotonic salt solution for raising the pressure. A cochlear manometer was used for direct recording of intralabyrinthine pressure. This pressure seems to be independent of the air pressure of the middle ear.

Culler, Finch, and Girden (**26**) injected saline solution intradurally in dogs and cats, and observed a decline of cochlear responses which varied in different trials from 2 to 27 db. No measurements were made of the intralabyrinthine pressure, but a marked bulging of the round-window membrane showed that this pressure was raised. The same procedure produced corresponding losses of hearing as tested by conditioned responses.

3. *Effects of Drugs.* Many chemical substances, such as cocaine, salicylic acid, chloroform, and strong saline solutions, when applied to the cochlea, have been found to impair its responses (**2**, **68**). Injection of these substances through the round-window membrane produces the most immediate effects, because the substances reach the essential tissues more promptly, and doubtless also because in many instances serious mechanical damage is done at the same time. Direct mechanical injury is avoided if the substances are instilled into the bulla, or if minute quantities in the form of drops or crystals are placed on the round-window membrane (**77**). The reductions shown by the cochlear response have been confirmed by conditioning tests (**55**, **57**).

Fowler and Forbes (**56**), after application of various substances to the round window, noted a progressive decrease of responses in order of frequency. High tones were impaired earlier and to a greater degree than low tones, and histological examination revealed a marked destruction of hair cells and sometimes of other tissues. The histological changes were usually most marked at the basal end of the cochlea, and the results were interpreted as supporting a place theory of cochlear action.

Wever and Bray (**159**) found the series of changes after application of sodium chloride to be somewhat more complicated. Frequently there is an initial augmentation of responses, which perhaps can be attributed to an alteration of electrical conductivity. The responses to all frequencies then suffer a rapid decline. However, in most cases the responses to high tones continue their decline for a longer time, and hence, after an interval, a systematic relation to frequency is established. The application of sodium chloride to a minute hole in the apex of the cochlea likewise causes general impairment, but of smaller degree.

XII. THE LOCALIZATION OF RESPONSES IN THE COCHLEA

The experiments just described, in which chemical substances are applied to the inner ear, have a particular interest for the problem of cochlear localization. Fowler and Forbes ascribed their results to a progressive involvement of sensory endings as the material diffused from the base of the cochlea toward the apex, a view that was supported by histological evidence of degenerations in the basal regions. They interpreted the results as evidence for localization in the cochlea, with high tones in the base and low tones in the apex. However, the results obtained by Wever and Bray complicate the problem somewhat, and if the place theory is retained some additional process must be postulated to account for the early losses suffered by all tones.

Other evidence on the problem of localization in the cochlea is gained in experiments on cochlear destruction, stimulation deafness, and the selective placement of electrodes in connection with the study of cochlear responses.

1. *Cochlear Destruction.* General and profound injuries to the cochlea invariably result in a cessation of auditory electric responses (**144**, **145**). Partial destructions sometimes produce great losses, but at other times cause changes that are surprisingly small. The differences are no doubt due to variations in the locus and amount of damage to essential structures. The smaller losses indicate that a part of the inner ear can continue to function after grave mechanical disturbances, including complete opening of perilymphatic and endolymphatic ducts. Bast and Eyster (**7**) reported that the apical, third, and second turns of the cochlea of the guinea pig could be removed without abolishing the responses. Potentials to low frequencies were markedly reduced, but those to high frequencies were only slightly affected.

Several attempts have been made to produce highly localized cochlear lesions. The results have varied, probably for technical reasons. Hughson, Crowe, and Howe (**91**) and later Hughson, Thompson, and Witting (**92**) reported experiments in which a drill hole was made part way through the cochlear wall, and the base of the hole scorched with a high-frequency cautery. They found most of the lesions to have the greatest effect upon high tones. Lesions in the apical region had relatively little effect. In chronic experiments in which several days elapsed between operation and testing, the impairment was more profound than in the acute experiments. Histological study of the chronic cases showed relatively diffuse atrophy of the organ of Corti and nerve fibers, possibly due to the spread of toxic products of the burned tissues.

Several experimenters have made simple mechanical lesions, and have observed an impairment of electrical responses (Van Gilse, **64**; Hinnen, **82**). Dubner, Gerard, and Kobrak (**46**) and later Kobrak, Lindsay, Perlman, and Dubner (**103**) observed the effects of cochlear lesions upon middle-ear reflexes, nerve potentials, and cochlear potentials, and found them to have the greatest effect upon the middle-ear reflexes and the least upon the cochlear responses. After a simple lesion, Bast and Eyster (**7**) found usually a loss of transmission of one-third to one-half, and after a time a rise to nearly normal. Culler (**23**) produced a lesion by drilling at the place that other explorations had indicated as the 'focus' for 200~. The results of both electrical and conditioning tests showed great loss of acuity for all test tones, and somewhat greater losses in the region of 200–250~.

Stevens, Davis, and Lurie (**136**) produced lesions by drilling into the cochlea in guinea pigs, and obtained impairments of cochlear responses which they regarded as correlated with the regions of destruction of sensory elements as later revealed by histological examination. The results were not given in detail, but were summarized in a figure which expressed the relation between cochlear damage and impairment of response. However, the sample data that were given show great irregularities, and the reviewer finds himself little in agreement with the authors' plottings of them.

It is evident that this type of experiment is far from satisfactory. It is difficult to produce a simple lesion because in a structure as fragile as the cochlea such manipulations as drilling and cauterizing, even though done with care, may cause injuries beyond the immediate site. Even if histological examination is made afterwards, it is difficult to evaluate structural damage and to differentiate functionally normal and abnormal cells. On the other hand, it is often surprising that a considerable amount of damage can be done without abolishing the cochlear responses. In this relation it should be borne in mind that the peripheral part of the ear is approximately a linear system, and a loss of half the elements active for a given stimulus should at most produce a reduction of about 6 db. The actual loss observed for a given amount of destruction will, of course, vary with the effectiveness with which the electrode taps the responses of the various elements.

2. *Stimulation Deafness.* Since the early work of Von Stein and Wittmaack, many experimenters have exposed animals to loud tones over varying periods of time in order to study the effects upon histological structures of the ear, and, in a few instances, upon the functional condition of the ear so far as it could be ascertained by the crude tests then available. Recently this type of experiment has been revived with cochlear response and conditioning tests to determine the extent of damage (**24**, **33**, **163**). The results of various investigators are in fair agreement. Tones of low frequency have little or no effect, while tones of 2500∼ or over sometimes produce impairments whose locations in the frequency scale bear little if any relation to the stimulating tones. In practically all cases there is a reduction of responses for tones over a wide frequency range (**164**), and a marked involvement of intermediate tones, presumably tones in the region of greatest sensitivity for the animal (**33**).

Results somewhat similar to those with tonal stimuli have been obtained with detonations (**92**). It has been reported that the cochlear responses for high tones are affected somewhat more than those for low tones, but these losses show no close relation to the site of damage as revealed in histological sections. There is evidence that, for pure tones and detonations alike, the most severe histological changes occur in the middle portion of the cochlea.

The relation of stimulation deafness to the effects already described under fatigue has not been made clear. It seems likely, however, that the 2 effects are successive stages of the same process of tissue damage.

3. *Effect of Electrode Position.* Hallpike and Rawdon-Smith (**76**, **77**) first reported a systematic variation of cochlear potentials as a function of the position of the active electrode on the bony wall of the cochlea. They found that as the electrode was moved from base to apex the responses to all tones progressively decreased. This change was attributed to the insulating properties of the bony capsule. However, by the use of a technique devised to neutralize the effects of insulation, they were able to demonstrate a differentiation of the responses with respect to frequency. At base and apex small holes were drilled nearly through the bone, and the recording

electrode made contact with a drop of mercury in the hole. They reported that responses to low tones were 3 or more times as strong when recorded from the apical electrode as from the basal, and, contrariwise, that high tones were about 4 times as strong when recorded from the basal electrode.

This observation has been confirmed by several investigators. Culler (**22**), indeed, has extended the observations by recording from a great many points on the cochlea of the guinea pig, and has presented a map which shows the focal regions of various tones from 60 to 7000∼. Unfortunately, only a few of the results upon which the map was based have been published, and these show considerable irregularities. Until a full report is made, or others repeat the work, it will not be possible to evaluate these observations.

4. *Nerve Sectioning.* An experiment of considerable theoretical interest is the selective sectioning of fibers of the auditory nerve. Three attempts at such an experiment have been made, but the results to date are not extensive. Wever and Bray (**156**) carried out partial sectioning of the eighth nerve in cats, and made observations of the effects upon responses from the contralateral cochlear nucleus (with the contralateral cochlea completely destroyed). The experiment is complicated by the presence of nonauditory fibers and the danger of cutting the cochlear artery. The results varied as expected; some sections caused no change, some produced a loss of the highest tones, and others showed a general and progressive loss of all tones. These results may be interpreted as indicating either a relatively specific representation of high tones in the nerve, or a requirement by such tones of a relatively large number of fibers. So far as they go the observations agree with those of Dandy on the effects of partial section of the eighth nerve in man for the relief of Ménière's disease.

Fromm, Nylén, and Zotterman (**59**) and Hughson, Thompson, and Witting (**94**) recorded from the cochlea, and observed no diminution of responses after severing successive strands of the nerve.

XIII. AUDITORY THEORY

The original incentive for the investigation of the electrical responses of the acoustic system was provided by questions of auditory theory. These questions, though not fully answered, are greatly clarified by results now at hand.

There is little doubt that the cochlear response represents the form of the stimulus as it is received and modified by the peripheral mechanism of the ear. Its study reveals limits of frequency, fidelity, and intensity for the transmission process—limits which account in part for limitations of auditory perception and response.

The transformation of intensity from a linear to a logarithmic scale, as subsumed in the Weber-Fechner law, does not appear in the cochlear response, and must be sought in further processes. As was brought out in Section VI, it is disputed whether this relationship enters between cochlear and nervous responses. At any rate, the evidence of Kemp, Coppée, and Robinson (**101**) indicates that a transformation of the required sort has already taken place by the time the lateral lemniscus is reached, though these writers themselves have not discussed this application of their observations. It was pointed out above that in their more regular functions the amplitude of response is proportional to the logarithm of stimulus intensity through the greater part of the range. Moreover, the departures from this relation at the 2 extremes of the function are in the same directions as the known departures from the Weber-Fechner law in hearing.

Two further problems lie in the foreground of attention. These are the spatial distribution of action in the cochlea and the representation of frequency and intensity in the auditory nerve.

Several results of the electrical studies bear on these problems. In the preceding section the evidence on the first was detailed. This evidence indicates a limited degree of differentiation of response in the cochlea with respect to stimulus frequency. High tones seem fairly well localized in the basal region of the cochlea, and low tones much less specifically localized in the remaining regions.

There is also evidence on the problem of the spatial representation of intensity. It is probable that all tones, and especially those of low frequency, are spread somewhat over the basilar membrane even when of moderate intensity. From the extensive impairments obtained in stimulation deafness experiments it seems certain that the spread of action increases with intensity, and that tones of great strength involve nearly the whole of the cochlea.

In regard to the second problem, we have as the chief feature of the auditory nerve response its representation of the frequency of the stimulus. It is agreed that below some limit, possibly 1000∼, the stimulus frequency is reproduced with at least fair fidelity by the individual nerve fiber. Above that limit the frequency is still represented, but only, it is presumed, by the combined action of many fibers. If refractory periods differ for the fibers set in action by a given tone, and if these fibers are unequally stimulated, their times of firing will not be mutually synchronous, but each individual impulse will be in time with some wave of the stimulating sound. The total pattern established by numerous fibers will properly represent the stimulus frequency. This, the volley hypothesis, accounts for the high frequencies observed in the nerve without requiring refractory periods below those known for other nerve fibers (Wever and Bray, **146**).

On this hypothesis, the highest frequencies so far observed in the nerve and in central tracts are not necessarily the highest represented. In the first place, the limits found are a function of the sensitivity of recording apparatus, and an improvement of sensitivity has usually revealed frequencies above those previously observed. A second condition is the type, size, and location of the electrode. It is obvious that an electrode so small as to record from only a few fibers will not present a true picture of the activity of a large bundle of fibers firing in rotation, and that this deficiency will be accentuated if fibers which represent essential parts of a pattern of frequency are spatially separated. The spatial separation of functionally related fibers will doubtless grow more serious as higher levels of the nervous system are reached. Finally, unless the transition from synchronism to asynchronism is particularly abrupt, it will be difficult to decide where the transition may be said to be, and also what degree of synchronism may yet have significance in the representation of characters of the stimulus.

The Place Theory. Opinions differ as to the theoretical bearing of the relations just indicated. Davis and his colleagues (**34**, **45**), apart from a few remissions, have adhered to the place theory. They have correlated stimulus frequency with the locus of response in the cochlea and hence with specificity of nerve fiber action, and have correlated stimulus intensity with the spread of action in the cochlea and hence with the number of nerve fibers in operation. They have regarded the frequency of nerve responses as an incidental affair, an epiphenomenon, so far as pitch and intensity are concerned, though it is admitted a possible rôle in the binaural localization of sounds.

Now, as throughout its history, this type of theory has had wide acceptance. A common argument is that the theory has the advantage of simplicity. However, as Wever and Bray (**146**) have pointed out, its simplicity is apparent only. A closer consideration shows that frequency of nerve impulses cannot be incidental to pitch perception even in a place theory. This condition arises from the fact that tones spread over the basilar membrane, and thus overlap so that the excitation of a given nerve fiber as such gives no clear clue to pitch. A definite clue can be provided only by some differential action of the various fibers involved in the response to a tone, to the end that not only the locus but also the form of the basilar membrane activity is represented. If the form is represented, then it will be possible through central processes to appreciate the pitch, in terms, perhaps, of the maximum point of stimulation as first suggested by Gray.

As is well known, the possible variations in nerve response are limited, and the only reasonable assumption for differential action of the fibers is through variation in the frequency of impulses. Pitch, then, on this supposedly simple theory, is a complex derivation of place and frequency. It is remarkable that these implications of the place theory have never been worked out by its adherents.

The Resonance-Volley Theory. Wever and Bray (**146, 156**) have developed the possibilities for auditory theory in a different direction, by a combination of fundamental features of the traditional place and frequency hypotheses. The evidence on the localization of action in the cochlea is accepted at its face value as indicating only limited representation in terms of place. Likewise, the evidence of the representation of tonal frequency in nerve activity is accepted as a significant feature of acoustic response. The volley hypothesis is used to explain the presence in the nerve response of frequencies much above those expected from a single fiber.

In this theory, pitch has a twofold representation. Low tones are represented by frequency, high tones by place, and other tones by a combination of the two. These suppositions follow the available evidence. Place alone serves for the high tones because localization in the cochlea for these is relatively specific, but the asynchronous character of the nerve volleys makes frequency representation indefinite. Frequency alone serves for the low tones, because they spread widely in the cochlea, yet in the nerve response they are represented with high accuracy. For the great body of intermediate tones both forms of representation are available.

Loudness is correlated with the total number of impulses in the nerve per unit of time, and thus relates both to the number of active fibers and the rates at which they enter successive volleys. These assumptions agree with evidence on intensity representation in cutaneous and other nerves, and embrace Adrian's intensity-frequency principle.

Other aspects of auditory experience that the resonance-volley theory explains include the number of cycles necessary for pitch perception, the variations of pitch discrimination with frequency, the resemblances between low tones and beats, and binaural localization in terms of phase. The theory brings together in meaningful relation the evidence obtained in the study of the electrical responses of the ear, and presents a consistent account of the various phenomena of hearing.

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PROGRESS REPORT OF THE COMMITTEE ON DISPLACED FOREIGN PSYCHOLOGISTS

BY BARBARA S. BURKS

Chairman

Following a vote at the business meeting of the American Psychological Association in September, a Committee was appointed by the incoming President, Professor Gordon W. Allport, to survey the problem of foreign psychologists whose livelihood has been taken away and whose safety has been endangered. The Committee, which provides regional representation in the United States and Canada (with a heavier concentration in the eastern states, where refugee psychologists are actually arriving in considerable numbers), consists of the following members:

Dr. Luton Ackerson, Institute for Juvenile Research, Chicago.
Prof. G. W. Allport, Harvard University.
Prof. W. E. Blatz, University of Toronto.
Dr. Barbara S. Burks, Carnegie Institution, Cold Spring Harbor, L. I.
Prof. D. B. Klein, University of Texas.
Prof. Gardner Murphy, Columbia University.
Prof. A. T. Poffenberger, Columbia University.
Dean G. D. Stoddard, University of Iowa.
Prof. E. C. Tolman, University of California.
Prof. Max Wertheimer, New School for Social Research, New York City.

Dr. Guenther Stern is an invaluable collaborator of the Committee. The aims of the Committee are to ascertain:

The present status, training, and qualifications of foreign psychologists who have been displaced from their positions through political developments of the past 5 years.

The possibilities of utilizing and conserving the potential contributions of these scholars to academic, social, industrial, and other institutions.

The points of view of the foreign scholars themselves as to how such contributions might be best effected.

To this end, members of the Committee have been collecting, and filing in the office of the Chairman, *curricula vitae* and relevant information concerning every psychologist who is an actual or potential refugee, and who could be reached by the Committee or by coöperating organizations. As it soon became evident that other committees were not specifically addressing the problem of scholars from other disciplines who have prominent psychological background and interests, the A.P.A. Committee also accepted for consideration a number of displaced scholars who do not belong exclusively to the field of psychology.

The Society for the Protection of Science and Learning (British) estimates in its November, 1938, report that "the total number of teachers and research workers displaced since May, 1933, from German universities and institutions of university rank is now approximately 1400." An additional 400 have been displaced from Austrian institutions of equivalent rank since "Anschluss" in March. Although the group with which our Committee is directly concerned constitutes only a fraction of the total group from all the disciplines, it may be valuable to view our problem against a broad canvas. The following statistics were taken from the November report just cited:

GERMAN SCHOLARS (exclusive of Austria)

Total Displaced	1400 (approx.)
Permanently Established Elsewhere	520
Temporarily Placed	290
Unplaced	600 (approx.)

It is seen that not many over one-third of the displaced scholars have found a semblance of permanent security, and that over 40% are "unplaced."

These figures take into account those scholars who were attached to universities or institutions of similar rank. Speaking now of the psychologists, we have a comparable group of about 50 persons, and also a number of highly trained individuals whose services have been rendered in extra-academic channels, who do not appear on the published lists of displaced scholars, but whose situation is fully as distressing as that of the academic group. Although our survey is not yet complete, and is far from complete for the non-German countries, comparative figures upon 78 psychologists for whom we have fairly adequate information have considerable interest.

Of 28 psychologists who have lectured or directed research in institutions of university rank, a *few* are adequately placed in various countries; 13 have found at least temporary placement (often very inadequate placement); 15 are unplaced.

Of 24 psychologists representing the "applied field" (including secondary school teaching and guidance), 5 have found at least temporary placement and 19 are unplaced. Selection may operate in this "applied" group, in that unplaced psychologists may be more often known to members of our Committee than those who are placed and no longer in need of advice and aid; our lists of academic psychologists, many of whom come to us through organized agencies, are undoubtedly more complete than those of the less systematically canvassed applied psychologists. It is the impression of the writer, however, that during the first difficult period in a new country, the relatively protected milieu of a higher institution of learning can be more readily adjusted to than that of clinics, industries, etc., where knowledge of the language and of the *mores* counts more heavily. Moreover, the academic psychologists are freer to emigrate than are those of the other group, who must wait for a "quota" visa.

In addition to these 2 groups of academic and applied psychologists, we have a group of 26 young university-trained psychologists who had been assistants, or who had not yet become established in professional work before conditions intervened to make their "habilitation" impossible. Of these, 9 are placed and 17 unplaced. Those who have come to our attention probably represent only a small proportion of psychologists actually falling under this heading. Several student psychologists have also been referred to us for advice as to how they could continue their studies—probably again only a small proportion of those whose studies have actually been discontinued.

We know of 2 psychologists who are in concentration camps at the present writing.

About 20 medical men, a dozen philosophers, and several scholars from related fields have been referred to the Committee—mainly men with research interests closely allied to psychology. Nearly all of these scholars are unplaced, or placed in unsatisfactory temporary positions. As the Committee did not originally attempt to survey these borderfields, it is safe to assume that these cases were referred to us chiefly in the hope of securing aid in placement.

The Committee is about to issue a list (with identifying data) of displaced psychologists and scholars in related fields. This will be sent for confidential use to universities and colleges and to other institutions on request.

With the task of "surveying the extent of the problem" partly but not wholly completed, and with a *few* scholars routed into positions through the efforts of its members (*e.g.* Professor Karl Buehler), the Committee now prepares to survey opportunities for placement. Bearing in mind the serious unemployment situation among American psychologists, the Committee is investigating the possibilities of "noncompetitive" openings, *i.e.* positions which may be newly created through the efforts of local academic committees which seek to assist their distressed colleagues, at the same time enriching the psychological thought of their own institutions; positions in regions or in countries where there is a visible undersupply of psychologists; positions in schools, social agencies, and industries in which the potential contributions of psychologists have only begun to be felt.

Funds for aid of the Committee's work to date have been made available through a loan from the National Coördinating Committee for Aid to Refugees and Emigrants Coming from Germany, through a grant from the Council of the American Psychological Association, and (mainly) from voluntary contributions.

Members and Associates of the A.P.A. are asked to communicate with the writer at Box 234, Cold Spring Harbor, Long Island, New York, if they have information on displaced foreign psychologists to file with the Committee. They are also urged to report openings, especially of a noncompetitive order, into which one or more of the refugees on our list might fit.

PSYCHOLOGICAL FACTORS IN MARITAL HAPPINESS

I.

TERMAN, L. M., assisted by P. Buttenwieser, L. W. Ferguson, W. B. Johnson, & D. P. Wilson. Psychological factors in marital happiness. New York: McGraw-Hill, 1938. Pp. xiv+474.

This volume sets out with the express intention of discovering the *correlates* of marital happiness. The presentation is characterized throughout by a strong sense of sin. Actually, the authors hope to find *determiners* of, *factors* in, *causes* of, and items *making for* said state, as well as an "adjustment scale" with predictive value. Periodically they urge the reader to remember that mere correlates, if any, have no causal implications; whereupon they feel free to indulge in exclamatory conclusions concerning the causal effects and influences of such correlates. But what is sauce for the reader ought also to be sauce for the authors. The procedure followed is that of the pious man who hopes to win salvation on the installment plan, interspersing spasms of wickedness between his recurrent confessionals.

Chapter 1 is properly cautious, defining happiness in operational terms as score on a specified questionnaire, and suitable reservations are made from extending the conclusions to different populations, times, and cultures. The more crucial question is the extent to which the inferences drawn actually apply to the population studied.

Chapter 2 presents the results of an exploratory search on 341 married and 109 divorced pairs, who submitted to modified Bernreuter and Strong questionnaires. General scores and detailed item reports were compared with score on a series of items calculated to yield a marital happiness index. The general trait scores showed little relationship, but some detailed items showed correlations that were felt to justify their later employment.

Immediately the causal question is raised: "Does marked husband-wife resemblance in these traits *make for* (italics mine) marital happiness?" And instead of "happiness results in one type of answers" we are told that "one type of mating favors happiness"; and this even when the traits are not measured until the marriage is of long standing, the divorce secured.

The correlation coefficients and other indices resulting from the "exploratory search" would have promptly brought to a halt less determined investigators. Table 2, for example, shows only 2 correlations (.20 and —.22) out of 48 that are 3 times their standard errors. But enthusiastic conclusions, terminating with exclamation marks, are found, such as (p. 23): "And how good a risk is the dominant male? The answer is that dominance in the husband correlates slightly positively with his own happiness but not with his wife's!" But the difference is only that between coefficients of .14 and .04, when the standard errors are over .05 and the "dominance" is only a Bernreuter score!

On the whole it would have been better either not to have published the 35-page report of this preliminary search, or to have put it also in the Appendix. It creates a strong suspicion of the 400 pages to follow, especially when, later on (Chap. 8), the reader is frankly told that such correlations as those here eloquently commented on indicate "a degree of relationship that is almost negligible." If this volume ever gets into the hands of the newspaper boys they will get a lot of fun out of the solemn discovery that it is unfavorable for happiness for spouses to disagree on their liking for Pershing, that it is ominous for both mates to feel the same way about crossing the street to avoid meeting someone, and that among the best differentiating items of happy from unhappy spouses are mutual interest in dental work and joint fondness for men who use perfume. Thus low have the personality traits fallen.

But we have only just begun, and the report of the main investigation starts out no less loosely. On page 46 "the purpose is to identify *correlates* of the happy marriage." But the next chapter, 2 pages later, begins, "On what psychological traits does marital happiness *depend* (italics mine)?" The data are the checked replies on a 12-page questionnaire by 792 willing couples, married on the average 11.4 years, and reasonably representative, it is thought, of the middle to upper-middle occupational, economic, and cultural levels of the urban population in California. However, the mean educational level was college junior and 674 of the individuals were college graduates or better.

Chapter 3 describes this fuller investigation, the methods of collecting the couples and securing anonymity, the information schedules, and the general plan of the investigation. Happiness indices for each individual were derived by totaling weighted scores from replies to questions about common interests, disagreements and their settlement, attitudes toward the marriage and the spouse, domestic grievances, and subjective estimates of happiness. These scores were markedly skewed in their distribution, piling up at the happiness end. The questionnaire contained, also, 3 other sections. One contained 233 "personality test items" (likes, interests, attitudes, response habits, opinions). Another contained questions concerning personal history, family, training, and background. The last set of items dealt specifically with habits and characteristics of sex experience and intercourse.

Chapter 4 is devoted to an analysis of the happiness scale and the interrelationships of the items used in deriving it. Chapter 5 undertakes to interpret the strikingly differential "domestic grievances" when happier and less happy marriages are compared.

At this point a serious methodological fault is encountered. Large critical ratios resulted when the grievance scores of happier and less happy individuals were compared—higher indeed than those found with other sections of the questionnaire. But the "interpretation," partly ex-cathedra and partly supported by internal contradictions, discards these high "correlates" on the ground that they are not causes but consequences. They are felt to be colored by the "halo" and "rationalization" due to marital mood. "Unhappy spouses display an irrestible (sic!) inclination to account for their unhappiness in terms of almost anything that is suggested" (p. 314).

There is no question in the reviewer's mind that this interpretation is sound. But, once admitted, it vitiates the whole procedure of the investigation. For the "halo" thus demonstrated infects also the replies to items of "personality," "back-ground," and specific sexual habits as well. There is evidence for such an outcome, besides mere assertion, in the literature of questionnaire technique. Nearly 20 years ago the effect of the Armistice was shown on the replies by invalid soldiers to an inventory not unlike those used in this study. The Armistice was found to produce a notable change in the report of neurotic and other complaints, amounting sometimes to a miraculous cure. Paraphrasing the present volume we could say of these results: "Unhappy soldiers display an inclination to account for their unhappiness in terms of almost anything that is suggested."

But the Armistice mood colored not only the report of personal complaints; it also strikingly modified the reports of family history and background. Quoting from the initial report of these findings:

> "The present condition of patients became enormously improved. Not only is this true, but even the facts of their history are more optimistically reported, and intermediate facts are intermediately colored. . . Before the Armistice the general disposition toward invalidism inclined the reports toward unfavorable perversion and pessimistic interpretation. After the Armistice the general urge toward release from military service led in much the same way, and probably to much the same degree, toward a favorable distortion of biography, heredity and experience, and a more optimistic interpretation than the facts would warrant in a report not definitely biased by motivation."

The analogy between marital ties and the bonds of military service is more than superficial. In the absence of clear evidence to the contrary it is fair to assume that marital and martial moods act in much the same way. If "the approach which relies upon direct questioning of the subject about his grievances can be and perhaps usually is grossly misleading" (p. 109), then it is doubly distressing that the personality indicators and case history data utilized in the 5 chapters to follow in the search for "causal factors" were secured in the same way. Not only are they mere "correlates," but they also may be expected to be "colored" correlates, perhaps just as "colored" as the domestic grievances. Even the combined "estimates of three psychologists" (p. 119) can do nothing to mitigate this misfortune. After all, it might have been safer and cheaper to rest with the main "theory," advanced in this chapter and later reaffirmed, that happy marriages are those made by happy people.

The ratios of specific complaints as between happier and less happy individuals are taken not as measures of any causal influences but merely as "rationalization indices," and it is declared that "viewed as factual testimony, the responses to the grievance items are next to worthless" (p. 95). In the light of this declaration the value of any other item reported in the questionnaire becomes infected with grave suspicion.

If the ratios of complaints are "too large to be accepted as credible evidence of fact," who is to decide when a ratio is to be accepted? If some data secured by a given questionnaire are to be dismissed as "incredible," what excuse is there for believing anything in those records? Surely, only an *obiter dictum,* such as has no place in a "metric" study. At the very least, some individual index, such as a "rationalization score," might have been used, which would probably come closer to being a revelation of "personality" than do many of the piddling items actually inquired about and given a "prediction weight."

Having shown that expressed grievances are "merely symptomatic," the authors turn in Chapter 6 to what they blithely call "factual information about the differences between happy and unhappy spouses with respect to their personality characteristics, their life histories, and their general background." Although, as we have seen, there is reason to suppose they have few or no such facts, still there are the answers to the questionnaire. "Happier" and "less happy" criterion groups were formed, comprising 300 and 150 of the total 792 couples, respectively. Based on comparison of the replies from these groups, and guided by the best judgment of 3 psychologists who tried to eliminate items estimated to be effects rather than causes, each item was assigned an appropriate, though arbitrary, weight as a component of a relevant personality scale. In Chapter 7 the personalities of happier and less happy mates are portrayed in various ways, using 132 differential items from the Bernreuter, Strong, and "opinion" lists. If the logical choice of items has any validity at all, then the inquiry itself was needless. If it has no validity, or only an indeterminate one, then the portraits given are equivocal. Moreover, they are usually in terms of probable trivialities, and at best are "ex post facto"—drawn after an average of over 11 years of marital endurance.

Chapters 8, 9, and 10 deal with "background" factors. The data are the replies to questionnaire items, with a value that is indeterminate because of just such halo influences as led war invalids to falsify their family histories. If factual data were available, these replies might be used to indicate *which* reports of background will be most notably colored by marital mood.

However, considering the items as reported, no significant correlations were found between happiness scores and income, occupation, children, religious training, birth order, sibling relationships, adolescent popularity, age differences, or schooling differences. Numerous items showed a small correlation, and are given small prediction weights.

Most related to marital happiness are parent happiness, childhood happiness, lack of conflict with parents, firm but not hard discipline, attachment to parents, parental frankness about sex, mild or infrequent punishment, lack of premarital aversion toward sex. These are given heavy prediction weights. It must be noted that these are precisely the kind of background items that could most easily be colored in memory by current mood. They represent mainly "complaints about parents" rather than "complaints against spouse." In fact, the rank for predictive value is probably the same as that for susceptibility to the "halo" effect.

Childhood happiness, for example, the authors admit is vulnerable to this error. Yet it is assigned high predictive and causal importance. It is even implied that marital happiness could be promoted by "making childhood happiness a universal birthright."

Chapters 11 and 12 consider the third main topic, namely, specific sexual adjustments, habits, practices, and techniques. The data from this section of the questionnaire are not called "personality" items. To the reviewer they appear to be at least more intimately *personal* than interest in Pershing, in dental work, or in pet canaries. The items cover a variety of questions relating to frequency, duration, enjoyment, time, and technique of intercourse, contraceptive practices, relative passion, orgasm adequacy, premarital and extramarital intercourse and inclinations, homosexual tendency, sleeping arrangements, etc.

Most of these, contrary to the sexologists, show no correlation with marital happiness. Some of the statistical findings would be of intrinsic interest if there were any way of knowing how representative these willing and patient married pairs were, even of their own culture group. The graphs picturing the approaching extinction of virginity at marriage, in both sexes, are an example.

Some of the items show sufficiently high critical ratios to be assigned weights in the prediction scale—though by no means proportionate to their differential values. Sexual complaints of spouse and sleeping in separate rooms, for example, are dismissed because "our own interpretation is" that they are results or symptoms rather than causes. Orgasm adequacy of wife and husband-wife equality or near equality in sex zeal give high correlations with happiness, and these, it is concluded, are "determiners." It is this mixture of ex-cathedra decision (however wise) with statistical findings, in which the wisdom always overrules the critical ratios, that the scientific reader will most regret.

Chapter 13 surveys the relative "contributions" ("correlates" has now been dropped) of personality, background, and specific sexual factors to marital happiness. It is maintained that the first 2 sets of scores, secured *before* the marriage, would be the same as when, 11.4 years later, "rationalization" and "halo" have done their work. Of this, as we have intimated, there is grave doubt.

The difficulty in determining what, if anything, these questionnaire returns mean, is illustrated by the way the authors wrestle with "infidelity of wife." The "opinion" data show this to be reported as undesirable or intolerable in over 95% of the cases. But the complaint lists show it to be less serious for husband's happiness than "not having meals on time." The opinion vote, it is felt, "may have been a parrotting of the traditional ideal rather than an expression of the real beliefs." And the grievances, as we have seen, although they have high discrimination values, have no real significance, because of the "halo." That is to say, the meaning of marital infidelity, if and when it should *really occur,* is wholly untouched by this investigation. So, the reviewer believes, is everything else. But this, one fears, will be overlooked by the feature writers, the teachers of marital adjustment courses, and the popular summaries in the pulp magazines.

The main point stressed is that these personality and background factors are as closely related to happiness as are the more strictly sexual scores. Arguments and some experimental results are given for the predictive use of the adjustment scale and comparison of its validity is made with that of other psychological techniques of prediction in this and other fields. Here also it is admitted that there is much overlap between what have been up to this point called personality factors and the reports on specific sexual items.

The last chapter gives a lucid summary and general conclusions. Four appendices give detailed analysis of the correlates of orgasm inadequacy, which remains a "mystery"; data comparing various special groups; percentile norms for the main portions of the scale; and the information schedule in full. There are 28 illustrative graphs, 147 tables, a bibliography of 114 relevant titles, and name and subject indices.

The volume contains a wealth of detailed analyses, correlations, differentiating values, critical ratios, and other comparisons, involving all the items here mentioned, and many others. It is full of instructive comparisons of these results with such conclusions as others have announced, with interesting, even if subjective, considerations of various possible interpretations of the results. The reader will find the discussion more interesting than this review might lead him to expect. Whatever its adequacy, the study is particularly timely in view of the current interest in instruction for marital adjustment and it will afford a mine of concrete material to replace the usual wordy content of such guidance courses. One trembles to think of the amount of probably misguided advice that will be administered on the basis of these provisional conclusions by people who miss the total picture and pin their faith to the details.

The prestige of the authorship and sponsorship will give this volume a halo of authenticity that will put its tentative intimations into the fireside journals as the final conclusions of science, and the science will be psychology, which already has a hard enough job defending its precarious status.

Finally, the main impression left by the book is that the authors are convinced that sexological, physical, and medical factors are less important determiners of marital compatibility than they have been said to be by the armchair authorities. The most important single determiner is surmised to be a psychological one, viz., temperamental and, probably in great measure, constitutional proclivity for happiness on the part of those entering into the relationship.

The authors acknowledge the tentative character of their results but emphasize the importance of further research in this field by such psychometric techniques as they have used. They are confident that "within a decade or two it will be possible by such methods to predict marital compatibility with greater accuracy than it is now possible to predict academic success from a composite of intelligence test score and previous scholarship records."

It might be suggested, as an afterthought, that the practical question is not how the happiness of one pair of spouses compares with that of another, but, instead, whether a given individual would (1) be happier married than single, and (2) which other individual, if any, he or she could most happily wed. On such issues the present results appear to shed no light. They are all *ex post facto*.

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II.

THE EFFECT OF HAPPINESS OR UNHAPPINESS ON SELF-REPORT REGARDING ATTITUDES, REACTION PATTERNS, AND FACTS OF PERSONAL HISTORY

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It has long been a matter of common knowledge that many varieties of personality-test items are invalid if the respondents know the purpose of the test and have something to gain by making themselves appear to be other than they are. The question has been raised whether the halo of domestic happiness or unhappiness of married subjects similarly invalidates self-report regarding attitudes, reaction patterns, and facts of personal history when the subjects not only have nothing to gain by coloring their reports but know in advance that neither the investigator nor anyone else can possibly find out how any respondent has answered a particular question. Light on this question is available from 2 sources.

In the first place, as is stated in my book on marital happiness (pp. 357–358), a partial check was made by comparing the correlations of personality-background factors with marital happiness separately for 2 groups so matched as to rule out irrelevant variables: (1) a group married 2 to 5 years and (2) a group married 15 years or longer. The point of this comparison is that, if responses of subjects to the personality-background items are seriously affected by the happiness or unhappiness which has developed in the marriage, these personality and background reports should correlate more highly with happiness scores in the long-married group than in the briefly-married group. The trend of the correlations was, in fact, slightly in the opposite direction. This finding suggests that responses to such items secured at the time of marriage, or even prior thereto, will yield similar correlations with later marital happiness.

More crucial evidence is furnished by a comparison of the responses made by 134 subjects in the post-honeymoon period (married less than 24 months) with those made by 284 subjects who had been married 6 to 8 years. We may, for the present, designate these groups as "A" and "C," respectively. As would be expected, the "A" group has developed little unhappiness. In fact only 6 of the 134 subjects in this group admitted that they were unhappy. The "C" group, on the other hand, had the lowest mean happiness score found at any interval for length of marriage. In the "A" group only 8, or 6.0%, have happiness scores below 55, as compared with 61 subjects, or 21.5%, in the "C" group. The difference is 5.2 times its standard error. Above the happiness score of 74 there are 65.9% of the "A" group and 45.1% of the "C" group. This difference is 3.8 times its standard error. So great is the contrast between the 2 groups with respect to happiness that we are justified in designating one as the "before" group, the other as the "after" group, meaning before or after unhappiness has had time to develop.

If self-reports by married subjects are greatly influenced by the halo effects of existing happiness or unhappiness, the responses of the "after" group on personality and background items should differ greatly from those of the "before" group. Such is not the case. Instead, as will be seen in Tables I and II, the distributions of responses by the 2 groups are practically identical. The "before" group, which had not had time to distribute itself widely on the happiness scale, answered the questions neither more nor less optimistically than the "after" group.

The 9 background items in Table I include those carrying the higher weights and account for about three-fourths of the total background score for prediction of marital happiness. The 17 personality items of Table II include 12 that carried weights of 2 points for one or both spouses (these being the heavier weights), and 5 that had been assigned reduced weights because of possible doubt as to direction of the cause-and-effect relationship. Both the background and the personality items chosen for this comparison are precisely those that would be most likely to be invalidated by halo effects of happiness or unhappiness.

On several of the items in Tables I and II outside evidence could be cited in support of my data. Consider, for example, the correlation between the marital happiness scores of my subjects and the rated marital happiness of their parents. Is this correlation an artifact produced by halo effects upon report, or is it true that marital happiness and unhappiness tend to run in families? In an attempt to answer this question Popenoe and Wicks[1] had the happiness of more than 2000 marriages rated not by the spouses themselves but by a third party who at the same time rated the marital happiness of the parents of the spouses. From their table of results I find the tetrachoric correlation to be .41 between happiness of the husband and of his parents, and .31 between happiness of the wife and of her parents. These correlations are higher than those yielded by my data (.25 for husband and .21 for wife). Comparing their findings with those of 2 earlier studies, the authors conclude that "these three studies point in the same direction and leave no doubt that happiness runs in some families, unhappiness in others."

Evidence to the same effect is available from my 16-year follow-up of more than 1200 "gifted children" in California. Up to about a year ago, 56 of the 608 marriages contracted by members of this group had ended in separation or divorce. Of these 56 subjects, 21.0% had parents who were separated or divorced. Of the 552 subjects in unbroken mar-

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[1] P. Popenoe & D. Wicks. Marital happiness in two generations. *Ment. Hyg., N. Y.*, 1937, **21**, 218–223.

TABLE I

COMPARISON OF "BEFORE" AND "AFTER" GROUPS ON RESPONSE TO BACKGROUND ITEMS

	"Before" Group (N=Approx. 130) %	"After" Group (N=Approx. 280) %	$\frac{\text{Diff.}}{\sigma \text{ Diff.}}$
1. Rated happiness of parents			
Above "average"	43.0	42.4	...
"Average" or below	57.0	57.6	...
2. Rated conflict with father			
Less than "average"	71.2	68.3	...
"Moderate" or more	28.8	31.7	...
3. Rated conflict with mother			
Less than "moderate"	74.6	71.4	...
"Moderate" or more	25.4	28.6	...
4. Rated attachment to father			
"Moderate" or less	41.1	47.0	1.1
More than "moderate"	58.9	53.0	1.1
5. Rated attachment to mother			
"Moderate" or less	37.1	43.8	1.3
More than "moderate"	62.9	56.2	1.3
6. Rated happiness of childhood			
"Average" or above	84.8	84.4	...
Below "average"	15.2	15.6	...
7. Childhood discipline			
"Strict" or "firm but not harsh"	63.3	68.8	1.1
Lax or irregular	36.7	31.2	1.1
8. Childhood punishment			
"Severely" or "frequently"	20.4	23.1	...
"Occasionally," "rarely," or "never"	79.6	76.9	...
9. Parents' attitude toward child's early sex curiosity			
Answered "frankly" or "briefly"	49.3	47.5	...
Lied, rebuffed, or punished	50.7	52.5	...

TABLE II

COMPARISON OF "BEFORE" AND "AFTER" GROUPS ON RESPONSE TO PERSONALITY ITEMS

	"Before" Group (N=Approx. 130) %	"After" Group (N=Approx. 280) %	$\frac{\text{Diff.}}{\sigma \text{ Diff.}}$
1. Often feel just miserable?			
Response *No*	56.0	63.9	1.5
" *Yes* or *?*	44.0	36.1	1.5
2. Frequently feel grouchy?			
Response *No*	57.4	60.5	...
" *Yes* or *?*	42.6	39.5	...
3. Frequently feel lonesome when with other people?			
Response *No*	64.2	65.0	...
" *Yes* or *?*	35.8	35.0	...

TABLE II—*Continued*

	"Before" Group (N=Approx. 130) %	"After" Group (N=Approx. 280) %	Diff. / σ Diff.
4. Often in a state of excitement?			
Response *No*	66.4	65.6	...
" *Yes* or *?*	33.6	34.2	...
5. Harder to be serene and cheerful than for most people?			
Response *No*	83.6	79.0	1.2
" *Yes* or *?*	16.4	21.0	1.2
6. Try to get your own way even if you have to fight for it?			
Response *No*	49.2	53.4	...
" *Yes* or *?*	50.8	46.6	...
7. Touchy on various subjects?			
Response *No*	51.5	51.9	...
" *Yes* or *?*	48.5	48.1	...
8. Useless thoughts keep coming into your mind to bother you?			
Response *No*	59.7	65.1	1.0
" *Yes* or *?*	40.3	34.9	1.0
9. Usually avoid asking advice?			
Response *No*	56.7	59.8	...
" *Yes* or *?*	43.3	40.2	...
10. Feelings alternate between happiness and sadness without apparent cause?			
Response *No*	53.8	61.3	1.4
" *Yes* or *?*	46.2	38.7	1.4
11. Critical of other people?			
Response *No*	47.7	50.7	...
" *Yes* or *?*	52.3	49.3	...
12. Does discipline make you discontented?			
Response *No*	51.5	54.2	...
" *Yes* or *?*	48.5	45.8	...
13. Feelings easily hurt?			
Response *No*	47.8	52.2	...
" *Yes* or *?*	52.2	47.8	...
14. Avoid saying things that might hurt anyone's feelings?			
Response *No* or *?*	28.4	27.5	...
" *Yes*	71.6	72.5	...
15. Disregard feelings of others when accomplishing an end important to you?			
Response *Yes*	30.6	27.1	...
" *No* or *?*	69.4	72.9	...
16. Lose your temper easily?			
Response *No*	76.9	71.1	1.3
" *Yes* or *?*	23.1	28.9	1.3
17. With opposite sex tend to be dominant and have own way?			
Response *No*	65.7	67.7	...
" *Yes* or *?*	34.3	32.3	...

riages, only 11.8% had parents who were separated or divorced, or little more than half as large a proportion.

Finally, in my marriage study I have compared ratings of parental happiness given by the entire group of subjects with the similar ratings by 72 of the subjects who had been married and divorced prior to the present marriage. In this comparison I have included only the previously divorced subjects whose spouses had not been married and divorced. We thus have a 2-directional comparison: (a) between parental happiness of previously divorced persons and parental happiness of the entire group (90% of them in their first marriage); (b) between parental happiness of previously divorced persons and parental happiness of their spouses (who had not contracted a previous marriage). The results are shown in Table III.

TABLE III

PARENTAL HAPPINESS RATINGS BY TOTAL MARRIED GROUP, BY SUBJECTS PREVIOUSLY MARRIED AND DIVORCED, AND BY SPOUSES OF SUBJECTS PREVIOUSLY DIVORCED

	Total Group %	Divorced Group %	Spouses of Divorced Group %
Rated Happiness of Parents			
Above "average"	48.6	37.5	50.0
"Average" or below	51.4	62.5	50.0

It will be noted that, whereas the per cents in column 3 are almost identical with those in column 1, the per cents in column 2 diverge markedly from those in column 1. The latter difference is approximately twice its standard error. Again we find that marital happiness and unhappiness tend to run in families. Halo effects have not appreciably affected the data on this question, as reported in my book. If the reader examines the text he will find a considerable amount of outside evidence cited on various other items relating to the personal history of the subjects.

The almost complete lack of halo effects in responses to personality and background items is in striking contrast to the presence of such effects in responses to items that call for specific complaints against the spouse or the marriage. In the latter, as I have shown, halo is present to such a degree that the complaints registered must be interpreted, for the most part, as rationalizations of the existing state of marital unhappiness.

Crucial information on the validity of the personality and background items I have used in the prediction of marital happiness scores is fortunately becoming available from a parallel investigation being carried out by Dr. Lowell Kelly under the same sponsorship that made possible my own. Dr. Kelly has applied a great number of physical and psychological measures to some 300 engaged and newly married couples and is charting, year by year, the course of each marital ship. For the sake of comparability of results he has secured from his subjects responses to the same questions on personality, background, and happiness that the Stanford study has utilized.[2] At my suggestion he has been kind enough to examine his material for such evidence as is now available regarding the validity of my happiness prediction scale, and to summarize this evidence in the note which follows.

III.

CONCERNING THE VALIDITY OF TERMAN'S WEIGHTS FOR PREDICTING MARITAL HAPPINESS

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As Dr. Terman has stated, he and I have been conducting simultaneous and somewhat parallel studies[1] of psychological factors underlying marital compatibility. The essential difference between the 2 investigations is that Terman's was cross-sectional, whereas mine is longitudinal or genetic. Terman obtained his information from married couples who were contacted but once *after* marriage. Almost all of the 300 couples being studied by me were interviewed and were administered tests *before* marriage. The success of the marriages is being evaluated by means of an annual follow-up blank similar to the scale of marital happiness used by Terman.

The personal-interview situation permitted the collection of considerably more data than was possible in Terman's investigation, but the long-time nature of my study makes it impossible to check the validity of the individual items until more time has elapsed. Fortunately, however, all of the items used by Terman were included in my study, and a preliminary check of the validity of his prediction weights is possible at this time.

At Dr. Terman's suggestion I have sorted out my distribution of responses to certain background items for the 300 couples who filled out my marriage schedule before marriage or very shortly thereafter. These include: happiness of parents, conflict with father, conflict with mother, attachment to father, attachment to mother, happiness of childhood, and childhood punishment.

Of the 7 items, 2 give distributions almost identical with Terman's. For the other 5 items the distributions, while not differing greatly from Terman's in the absolute sense, diverge enough from his to make the difference statistically reliable because of the rather large N's in the populations compared (approximately 280 and 580). The divergences could easily be due to cultural differences between New England and California, or to selective factors operative in "willingness to coöperate" in the 2 studies. One of the 5 reliable differences (conflict with mother) is, in direction, contrary to the reviewer's criticism.

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[2] This statement is not to be understood as implying that the Stanford study has any claim to priority over Dr. Kelly's. They were conceived at about the same time and it was only a matter of chance that my information schedule was ready for use at the time Dr. Kelly began the collection of his data.

[1] Both studies were made possible by grants from the Committee for Research on Problems of Sex of the National Research Council.

Much more pertinent is the question whether Terman's prediction measure actually predicts happiness for my subjects who answered the questions before there was any possibility of marital unhappiness being present. I have computed the total prediction score (personality plus background) for 82 of my couples who have been married 2 years, and I have computed for these couples the correlation of the prediction score with marital happiness score at the end of 2 years. The resulting raw correlations are .26 for husbands and .30 for wives. When adjusted to allow for the extremely narrow happiness range of my group, these become .50 for husbands and .56 for wives. The corresponding correlations reported by Terman for 200 couples were .54 for husbands and .47 for wives.

In spite of the theoretical objections to assigning "prediction" weights on the basis of mere "correlation," Terman's weights seem to be valid for an entirely different population, which responded to the items before marriage.

BOOK REVIEWS

THURSTONE, L. L. Primary mental abilities. Chicago: Univ. Chicago Press, 1938. Pp. 121.

This monograph is the first of the series sponsored by the Psychometric Society. The author's purpose is to "describe the fundamental ideas of factor analysis in terms of only the simplest mathematical ideas in the belief that some readers who are not interested in the mathematical proofs of the previous volume (*The vectors of mind*) may, nevertheless, have some interest to know the essential nature of these methods." In this he succeeds quite well, with the single exception of Chapter IV, which is probably still beyond the average ability of those uninitiated in factorial mathematics. The general effect of reading this book after laboring through *The vectors of mind* is very similar to the rather satisfying experience one has upon looking at the completed structure after having toiled through the blueprints.

The general purpose of factorial analysis is to describe succinctly and adequately the individual differences in human abilities by finding the several primary abilities which underlie human behavior. The necessity for such an analysis is dictated not only by the economy of effort and attention which the results would provide, but also by the deeper understanding which it would give of all human abilities with special relationship to the influence of genetic factors as well as cultural factors.

Ever since it became apparent that factorial analysis does not lead to a unique result, but that there were an infinite number of results possible, workers in this field have devoted their attention to finding some criteria for selecting one result rather than another. The criteria that have been proposed thus far are: invariance of results, psychological interpretation of results, and practical utility of results.

In order that the results be invariant, the factorial structure of a given test must remain constant from one analysis to another. This is often not the case in the results obtained thus far. Sometimes the introduction of a new test or the removal of an old test changed the factor loadings (*i.e.* correlations between tests and factors). These correlations were also found to depend upon the sample of individuals tested. It is clear that if the correlations between a given test and a factor are to be stable, they ought not to change from one situation to another, or, if they do, adequate explanations for such changes must be provided.

In order that the results may be psychologically meaningful, some consistent hypothesis must be available for explaining the nature of the factors. Tests that correlate highly with a given factor must have something in common which is psychologically understandable. Unless some psychological explanation of this common bond is available, the results cannot be regarded as meaningful. Then, also, negative factor loadings crop up in many results. How a factor can affect a test by subtracting from it rather than contributing to it is difficult to comprehend.

In order that the results have practical value the factor scores, *i.e.* the relative amount of each factor possessed by a single individual, must be utilizable in the description of the individual's ability or behavior. This, in the final analysis, is the purpose of all measurement. It is not sufficient for the results to be psychologically meaningful in the sense that they are explicable in terms of some hypothesis. The factor scores should also serve the same end as do test scores, but should serve it in a much better way. Unless the factor scores turn out to be superior to the original test scores, why spend all the time and effort in factor analysis?

The first two conditions, invariance and meaningfulness of the results, are declared by Thurstone to be dependent upon our ability to find the simplest factorial structures for the test under analysis. This simple structure is to be obtained by first analyzing the correlation table by means of the centroid method and then rotating the axes of the obtained factors graphically until the number of zero factor loadings for the tests (*i.e.* the number of tests that are independent of each factor) is maximized and the number and size of negative correlations with each factor are minimized. The reason for wishing to minimize the number of negative correlations is obvious. The purpose of maximizing the number of zero correlations needs more examination.

It is to be expected, Thurstone points out, that some tests will have zero correlations with several of the factors, for it is probably impossible to create a battery of tests each of which will encompass all the psychological factors that may be operative. Taking this as his cue, he rotates the axes for each pair of factors in such fashion that the number of zero loadings is maximized. In other words, each test can be regarded as made up of as few factors as possible. One can readily follow Thurstone in his first statement, but his conclusion that the number of zero loadings must be maximized does not seem to follow as a necessary result. Furthermore, the assumption that a simplified structure will remain more invariant than any other type of structure is an assumption that requires more empirical proof than is now available. But even granting that Thurstone's simplified structure will render the factor matrix independent of the removal or addition of any single test, the factorial structure may yet be altered by other means. One of these is the characteristics of the individuals in the sample. Thurstone pays little attention to this source of possible invariance, although there are studies pointing in that direction.

One necessary result of the rotation of axes and the maximizing of the number of zero factor loadings is the elimination of the general factor running through all the tests which Spearman postulates. It is therefore somewhat difficult to follow Thurstone's conclusion: "So far we have not found the general factor of Spearman, but our methods do not preclude it." If conscious rotation to eliminate the general factor does not preclude it, what will?

The third condition, that of practical usefulness, is dealt with in the chapter on the interpretation of the factors. In this section Thurstone gives the net results of the application of the factorial analysis to the fifty-six tests, the work having taken several years. This chapter reveals most enlighteningly the long road that factor analysis still has to travel before it becomes useful to the practical psychologist. Perhaps the most difficult problem of all is the problem of describing the factors after they are isolated. The usual procedure is to take some arbitrary factor correlation like .40 (the limit used by Thurstone) as the critical value. Tests that do not correlate with the factor to the extent of more than .40 are not considered in the determination of the character of the test. This procedure is, of course, highly arbitrary, and it is doubtful whether unanimous agreement could ever be reached on the proper name or content of a given factor from a knowledge of the tests with which it correlates. Indeed, Thurstone himself finds difficulty in explaining the presence of high factor loadings for some tests and their absence in the case of others. In one or two instances some unusual *tour de force* is resorted to in order to explain the factor loading of a test. For example, in order to explain the high visual or spatial factor loading of the syllogism test, the author has to resort to the following: " The essentially visual character of the syllogism test is readily seen by inspection. The premises are concerned about the relative ages of three individuals, and the simplest way to keep them in mind is to represent them by lines or relative elevations. This is the way most people solve these syllogisms, and hence facility in visualizing augments the score." It is at least questionable whether most individuals utilize this type of visual imagery in syllogistic reasoning. The procedure of inferring the nature of the factor from the tests with which it correlates is a hazardous task and is in great need of supplementation by more objective devices, if any can be found.

The technique, disappointing as it is from the point of view of immediate returns, nevertheless seems to hold promise for the future. The succinctness of the approach must command the respect of all of those who become acquainted with it. The rapidity with which improvements are being made and difficulties removed is also striking. One source of difficulty that has often confronted previous work in factor analysis was the question of where to stop in the determination of factors. There was no way of telling whether all the factors had been extracted. No mathematical solution of this problem is yet available but an empirical device for determining whether or not the number of factors is complete has been evolved.

The rotation of axes at the present time is conducted in a graphic, nonmathematical manner. It is hoped that within the next few years a more rigorous mathematical procedure for performing analytically the work which is now done graphically will become available.

Despite the rapid advance that factor analysis has made in recent years, there is still much left to be done. Thurstone realizes the basic inadequacies of the present status of factorial procedure and in some instances is quick to point out these difficulties as well as possible remedial methods for the future. Perhaps one of the most outstanding difficulties is the question of the linearity of relationship between the original variables. Thurstone side-steps this issue completely by discarding the actual distribution surface and substituting for it a dichotomous distribution for each variable, the dichotomy being drawn at the line of the medians. This is done not only for the relationship between the variables but also for the reliability of each test. The tetrachoric correlations that are obtained in this manner are, of course, dependent upon the normality of the distribution surface as well as its continuity and on the linearity of the regression line. Whether these assumptions are at the basis of any of the difficulties that Thurstone finds in interpreting the results is well worth investigating.

A second difficulty that Thurstone recognizes is the apparent non-significance of the individual profiles that emerge when the factor scores for each individual are determined. One wonders whether this result is not in part due to the above-mentioned artifact in the utilization of tetrachoric correlations. Perhaps a second reason for this result is the fact that the factors themselves are independent by definition. In other words, the coexistence of any combination of factor scores in a single individual is purely a matter of chance. Small wonder, then, that no definite patterns tend to appear. They are made impossible by the nature of the factor scores upon which they are based.

Some of the difficulty which is being experienced in factor analysis today might be obviated if, instead of proceeding in the direction of factorial analysis of traits alone, we would supplement it by factorial analysis of individuals; and, indeed, Thurstone points out a recent trend in that direction. However, those attempts, especially those of Stephenson and Burtt, are posited on a statistical basis which presents many difficulties. Stephenson and Burtt use as their starting point the correlation between pairs of individuals instead of the conventional correlation between pairs of tests. The correlation between two individuals considers each of the two individuals as a "test" or "trait" and the pairs of scores on each of the different tests as belonging to different "individuals." However, one of the assumptions of any distribution in a correlation surface is that the frequencies of the scores must be independent of each other. It is difficult to see how a score on one test can be independent of a score on another test made by the same individual, especially if the correlation between the two tests is significant. A second difficulty, which perhaps is inherent in the first, is the impossibility of finding a continuum on which to distribute the scores of the same individual on different traits. Still a third difficulty is the selection of the ideal for each type which is to serve as the basis of correlation. The selection of the ideal individual is, at best, arbitrary at the present time.

The success that Thurstone's approach has won thus far seems to be limited to the field of mental abilities of the intelligence test type. He admittedly finds little success in the field of personality and vocational guidance, and is pessimistic enough about the personality tests of the paper-and-pencil variety to state that they are doomed to extinction. He expresses similar opinions about group tests in general and indicates that the future belongs to individual tests. It is interesting to speculate on the causes of this failure in the field of personality tests. Perhaps one of the reasons of the lack of success is the empirical check that the clinical field affords. It makes very little difference at the present time, practically speaking, whether we regard a given test as made up of g or of s or of v. However, if some personality factors of an observable kind are postulated in an analysis, it soon becomes apparent whether the factors correspond to observed behavior or not.

Perhaps a second reason for the failure in the field of personality is the inadequate standardization and validation of these tests. As long as most of these tests are validated in an *a priori,* logical manner rather than in an empirical way, one cannot hope to obtain psychologically meaningful results from them.

A third difficulty that lies at the basis of the failure is perhaps fundamental to the entire factorial approach. Factorial analysis is primarily an attempt in the classification of human abilities and, in the last analysis, will stand or fall on the degree of usefulness which the classification system will provide. The assumption is made that a frame of reference exists in which human abilities can be described in terms of a comparatively small number of continuous, normally distributed, independent factors. Perhaps such a frame of reference exists, but if it does, it defies some of the concepts now current in psychological thought. It is difficult to make such a system compatible with Gestalt theory or with typology. Independent factors cannot give rise to any specific patterns or structures in individuals since independence precludes anything but a chance relationship in the distribution of factor scores in a given individual. Gestalt and typology ought at least to be given a chance, and the present factorial attack precludes this chance. Perhaps by starting at the other end of the task and beginning with groups that are recognized as socially distinct, as, for example, delinquents and nondelinquents or successful and unsuccessful teachers, we may supplement Thurstone's approach and help crystallize the meaningfulness of the factors that have thus far been found. If, instead of throwing our full energy into the task of finding the primary abilities of man, we were to spend part of our time trying to discover the differential symptoms or syndromes of symptoms that distinguish contrasted socially recognizable groups, we might, as a result of the latter analysis, aid in the determination of the intrinsically primary abilities of man.

It is apparent that Thurstone has given us a very sharp tool which can cut keenly into our psychological problems, if used wisely. At present the tool is far sharper than it is useful. How to render it more useful is indeed one of the outstanding problems facing differential psychology. Perhaps by combining the contributions of differential psychology with those of typological psychology, both methods will be benefited and present-day psychology will be brought out of its chaotic condition.

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KALLMANN, F. J., with the assistance of S. J. Rypins (with an introduction by N. D. C. Lewis). The genetics of schizophrenia: a study of heredity and reproduction in the families of 1,087 schizophrenics. New York: Augustin, 1938. Pp. xvi+291.

The basic material of this study consists of all the schizophrenic case histories available in 1929 in the archives of the Herzberge Hospital in

Berlin from the first ten years of its existence (1893–1902). Four years of field work were devoted to the collection of reproductive and psychiatric data about the ancestors and descendants of the patients ("probands") and about their siblings and the children of the latter. The principal objects of the survey were to ascertain the level of fertility and mortality of the probands and their various descent groups, and the incidence of psychopathy among them; and, where control data (from other studies) existed, to make the comparisons. Secondary projects were a description of the ancestry of the probands, a determination of the incidence of psychopathy other than schizophrenia and "schizoidia" in the descent groups, and a study of the relationships between schizophrenia and tuberculosis. The work is furnished with the usual scholarly paraphernalia, of which a large bibliography is the most important.

It is hardly necessary to say that the work is executed with Teutonic thoroughness, and at once takes its place as one of the fundamental sources on the inheritance of psychopathy. The results are largely a matter of how you look at the problem. One way of looking at it, which we may call the eugenicist's view, is to focus attention on the expectation, in the statistical sense, of developing any form of psychopathy, given membership in a specified descent group; at the other extreme is what we may call the view of the common man, who wants to know the answer to such practical questions as whether his children by the niece of a schizophrenic would have any substantial likelihood of incurring mental illness. It makes a big difference. The answer to the eugenicist's problem is that "types belonging to the schizophrenic disease-complex" have an expectation of 44% in the children of schizophrenics, 25% in their grandchildren, 3% in their great-grandchildren, 19% in their siblings, 14% in their half-siblings, and 9% in their siblings' children. The answer to the common man's problem is that the taint of 1087 schizophrenic patients will be represented in their grandchildren's generation by eight definite schizophrenias. The difference is, factually, primarily a matter of the low reproductivity and high mortality of schizophrenics; statistically, it seems to be a matter of attempting to estimate "how many secondary cases would have occurred among these individuals if they had all survived the danger-period for schizophrenia."

The secondary results, as indicated, include clear determinations of fertility (53% of the probands had no children, 77% no grandchildren; the whole proband group had 2000 children and 1016 grandchildren—in a precontraceptive age) and mortality (about half of all the children of probands and nearly a quarter of their grandchildren died before twenty). They also include: evidence of a preponderance of tuberculosis among the causes of death; indications that nonschizoform psychopathy, feeblemindedness, alcoholism, and criminality (except as a consequence of schizophrenia itself) are not associated with schizophrenia; and a conclusion, admittedly limited by the difficulties of field work on people long dead, that hereditary predisposition to schizophrenia was present in at least a third of the probands' parents, uncles, and aunts. Chapter V is devoted to protocols of secondary and special cases, and affords a sample of the diagnostic criteria in the more doubtful instances. Incidental findings of interest are that, while there are practically no sex differences, the "nuclear" schizophrenias (catatonic and hebephrenic) behave in a sharply different manner from the "peripheral" group (paranoid and simplex), and that about 70% of what fertility there is is prepsychotic, so that only a very slight reduction, beyond that effected by nature, could be expected from segregation or sterilization.

It seems entirely reasonable on the basis of the evidence presented to believe that schizophrenia is partly conditioned by general tissue defects having germ-plasm determiners. (To the author this seemed obvious from the beginning, and as a result of his study he adds to this basic hypothesis that the transmission is Mendelian recessive and involves a single gene pair, and that the tissue affected is the reticulo-endothelial system.) Nevertheless, there are certain logical precautions which are regularly missing in medically conceived investigations of this sort, and which, in view of the importance of the problem and the growing maturity of the field, it does not seem excessive to suggest. The most important (and most difficult) of these is control against possible pathogenic effects of environment in early childhood. We have strongly suggestive evidence from several sources that the young child learns his emotional habits, as he learns other habits, from the only source available to him, namely, the adults with whom he lives, and it would be surprising if the emotional habit system called schizophrenia should prove an exception to this rule; at any rate, in an investigation designed to be precise it would do no harm to introduce this control. A further possible source of error is lay diagnosis, which, as in the case of subjects long dead as recalled by naïve or elderly persons with every conceivable emotional distortion, may be very crude indeed; it is at least doubtful whether the superficially impressive size of the notorious Jukes material, for example, has not been more than counterbalanced by its profound social effect in fixing in the minds of hundreds of thousands of students a conclusion which, whether true or not, is certainly of extremely low probability. A third difficulty is lack of control against possible cultural pathogenic pressures, and this may be looked upon as only one aspect of the general problem of normal controls. It is convenient, but hardly rigorous, to compare an experimental sample of this sort with some other investigator's supposedly normal sample drawn, perhaps, from another social class in a different time and place. At this stage of our knowledge it would be far better research strategy to match each proband with a known nonschizophrenic of the same sex, approximate age, socioeconomic status, and possibly family background, and then to investigate both by precisely the same techniques. Finally, while we have the customary respect for experienced and professional diagnostic competence, this purely affective support does not seem the most secure imaginable for conclusions of this weight. A psychologist observing the behavior of a child, or an examiner marking a test, does not rest content with assuring the reader of his report that he exercised the greatest possible care; rather, he presents the evidence, such as the percentage of agreement with another observer working independently, from which the reader may judge the reliability of his observations. A good deal of spadework in this region is probably propaedeutic to this kind of investigation. In the first place, we need some sort of yardstick for the schizophrenic behavior continuum, analogous to a scale for the grading of handwriting; such a yardstick could probably be made up of units of a few hundred feet of sound-movie film, duly edited by a group of experts to represent whatever might be agreed upon as approximately equal increments of severity. Studies could then be made of the reliability of assignment of specific observations to positions on the scale, and observers could be trained to a desired criterion of expertness before being sent into the field to observe descendants of schizophrenics and their matched normal controls.

What we are suggesting, in brief, is increased statistical, *i.e.* logical, sophistication in the design of important studies of this sort. It is true that the size of the material examinable within given limitations of time and expense would be enormously reduced. What we seek, however, is increased probability of conclusions; and contemporary statistical theory has progressed beyond the point of ascribing this chiefly to increase of populations.

R. R. WILLOUGHBY.

Brown University.

LANDIS, C., & PAGE, J. D. Modern society and mental disease. New York: Farrar & Rinehart, 1938. Pp. xi+190.

If *multum in parvo* were ever appropriately applicable to any book it is to this one. It is in essence a huge handbook boiled down into a few pages, and a tremendous amount of labor must have gone into its production. For all this, workers in many fields will long be indebted to its authors. For information on any and every statistical aspect of mental disease there is not likely to be a better reference text for many years. Psychologists, sociologists, biologists, statisticians, geneticists, and eugenicists must all use this summary for convenience and for the last, the most up-to-date, word on many topics. There is material here for the teacher and for the researcher, and there is material here to make every scholarly person pause and consider. The authors have apparently ransacked the earth for their information. There was the searching of all available books and journal articles; government reports from more than a dozen countries were examined; and there was correspondence with many officials in many countries. For all of these sources a useful list appears in the Appendix.

One would have to reproduce the book to reproduce the findings of the authors. But, in all too imperfect summary, that most interesting to psychologists is their conclusion that the so-called psychogenic theory of etiology for the functional psychoses is without adequate foundation. The evidence, as summarized, points to an intrinsic or constitutional defect of some sort at their base. But this does not mean that these diseases are so clearly hereditary that a simple eugenic program would serve to stamp them out. Rather, it is made clear that in the light of our present knowledge (or ignorance) there is little justification for a wholesale eugenic program by the sterilization route.

Obviously, no teacher of abnormal phenomena and no investigator in the field can afford to ignore this volume. Most will find it an indispensable *vade mecum*.

EDMUND S. CONKLIN.

Indiana University.

GILBERT, M. S. Biography of the unborn. Baltimore: Williams & Wilkins, 1938. Pp. x+132.

As might be guessed, *Biography of the unborn,* written by an embryologist, is a description of the development of the human being from the time the minute ovum and microscopic sperm unite in the creation of a human life until the child emerges from the parent body.

While the story is narrated chronologically—a chapter for each month, the author has artfully and skillfully woven her tale so that one aspect of development is completed to the satisfaction of the reader before another aspect is begun. Such a simply told story of one of the most intricate processes of nature could only be written by a person who knows her subject matter thoroughly and who has also integrated the facts into an analyzed, but schematic, whole. The lucid exposition does not oversimplify the mechanics of development but, instead, serves to emphasize the complexity and marvel of life mechanisms.

Each of the ten chapters from "Genesis" to "Exodus" stresses the most characteristic aspect of development for the successive stages of growth, but continuity of exposition is maintained by recurrent reference to the various phases of morphogenesis. Finally, Chapter 11 discusses twinning and abnormalities of growth. A list of suggested readings and a glossary complete the book.

From a psychological point of view it is unfortunate to conclude with an emphasis on abnormalities, particularly since the book is written for the layman and will undoubtedly hold particular interest for the expectant mother. To read of harelips and one-eyed monsters can only serve to make her anxious. However, it is difficult, *a priori,* to judge the reactions of other nonscientific persons to this chapter. Instead of causing horror and anxiety, the explanation may make the abnormal appear so normal that marked physical defects lose some of their stigma. In any case, it would have been better, in my opinion, either to have completely omitted reference to growth abnormalities or to have ameliorated their discussion by including them at appropriate places in the description of normal growth. But it is unfair to be overcritical on this point when the work as a whole is of unquestioned value; it deserves high praise.

As a contribution to psychology, the book might well be required as underpinning for all students—particularly students of child psychology, because it presents undeniable and concrete evidence of the progressive patterning and repatterning that occurs in development, whether behavioral or physical. Students of general psychology are too unaware that it is only by examining the processes of behavior development that one can fully understand its mature expression; and students of child psychology are too likely to consider behavior development as merely more skillful functioning. They fail to realize that just as three kidneys are formed and discarded in the growth processes of fetal life, so many child behaviors are formed, discarded, and built anew to meet the successive stage of growth.

Dr. Gilbert's book is a valuable contribution to the dissemination and humanizing of scientific phenomena.

HELEN THOMPSON.

Clinic of Child Development, Yale University.

CURTI, M. W. Child psychology. (2nd ed.) New York: Longmans, Green, 1938. Pp. vii+458.

In 1930 the first edition of Dr. Curti's *Child psychology* was published, and for two academic generations it has been a familiar text in many college courses. The present reviewer must frankly confess that the first edition has been to him just another book on child psychology—not a particularly good one, but still not very bad—just mediocre. With this judgment the second edition stands in vivid contrast. In the Preface of the new volume Dr. Curti states the hope that her psychological thinking has matured. The affirmative evidence that it has is repeatedly available in the text.

To those familiar with the first edition a formal appraisal will be significant. While the new book is only 2% longer in number of pages, a considerable change in typographic style has allowed a real increase in content of 13%. Even these figures do not fully represent the quantitative change. The chapter on juvenile delinquency, which had no real place in this book, has been eliminated. The chapter on play has been omitted as such, but certain pertinent material has been made part of a more general chapter on problems of mental life. The material on heredity has been rewritten so that pure biology is minimized and there is a real attempt at a critical appraisal of the parts played by heredity and environment in the child's development. Two chapters have been added in which the problems of individual differences and general intelligence are discussed. Following a rewritten "Introduction" there is a new second chapter which presents a condensed history of one child's development. This history serves to give the student a concrete picture of what the author is planning to discuss and is used throughout the text by way of illustration. Another valuable addition is a brief bibliographic note at the end of each chapter evaluating several books pertinent to the content of the chapter.

The contents of this edition systematically organize the picture of behavior development. A mere enumeration of chapter topics illustrates this. The biological organism—with due emphasis on hereditary, intrauterine, maturational, and physiological factors—is discussed in three chapters which depict the newborn and what he has to build with. Next, the general problem of individual differences in various behavior functions, including general intelligence, is treated in two chapters. With the groundwork laid, the processes of behavior change are shown to take place by learning (the conditioned response, complex habits, growth of meanings, including speech) until the complexities of human behavior are possible. The discussion of the more complex behavior includes reflective thought, social behavior, and, finally, the formation of personality. It is evident that this organization is much more logical than that of the first edition. Furthermore, the author has carefully reviewed every section of the old book, omitting or revising those parts which newer research has changed or made more clear, but retaining the old that is good. Nearly half of the references cited are to material published since 1930, the date of the first edition.

Perhaps the point to be most admired in the new text is the consistent, but not slavish, adherence to a point of view. Too many books on child psychology are either compendia of experimental abstracts, eclectic compilations interpreted in words of one syllable, or outright child study manuals written for a nonpsychological public. Dr. Curti is fundamentally a Chicago functionalist, and it is toward this theoretical position that she orients her work. However, she lives up to her own advice to students: "No matter how valuable a consistent point of view is, as an aid to thinking, it may become a hindrance if not held in the true spirit of science—critically, open-mindedly, humbly—as a tool for the more effective pursuit of truth." Systematically, this book is consistent without becoming fanatic; in contrast to the first edition it is a well-organized and carefully thought-out exposition of the development of child behavior.

C. M. Louttit.

Indiana University.

Harrower, M. R. The psychologist at work: an introduction to experimental psychology. New York: Harper, 1938. Pp. xiv+184.

If the title, "The Psychologist at Work," should mislead anyone into conceiving that this volume is concerned with the impure psychology, the contents of the early pages will soon disabuse him of the notion. He is told unequivocally (p. 3) that "psychology as a branch of learning has no concern with bettering the worldly or economic status of the individual." Having thus decapitated psychotechnology at a single stroke, Miss Harrower rubs salt into the wound by implying that the only psychology other than the 'academic' is that which attempts to "guarantee eloquence, personality, magnetism, charm, wealth, confidence, remarkable memory . . ." Thus vanish the pretty conceits and vain pretensions to sound methodology of Mayo, Viteles, Hull, Johnson, Strong, Lahy, Moede, Myers, Muscio, and their like, while the A.A.A.P. takes its proper place as an artisans' union. Miss Harrower is, alas, but following tradition when she suggests that there is and can be no 'branch of learning' concerned with the practical aspects of man's life. One wonders how much longer modern positivistic psychotechnology must knock at the temple gates before it gains admission as a research discipline worthy of a rear pew at great remove from the altar.

To return to our muttons, Miss Harrower has succeeded in writing an abridged account of psychology as she conceives it and, however much one may question the catholicity of her definition, one may not question at all the dignity and insight with which she has treated the fields included in her definition. In 175 pages she manages to present stimulating and accurate descriptions of basic facts and basic methods in perception, in the genesis of behavior, in emotions, in remembering, and in learning. Her exposition shows clearly the impress of her connection with *Gestalt* psychology without, however, becoming at any point a plea for that point of view. Most of her references deal with publications of the last twenty years, but she is not averse to citing earlier researches.

The shortcomings of the volume (other than its conventional, but arbitrary, restriction of field) are less apparent than its virtues. When one deletes the physiological studies from her chapter on "The Emotional Side of Life," one is left with an impoverished fabric which probably represents the state of the field rather than a weakness in the author's treatment. Likewise, the attempts to 'explain' observed psychological facts by reference to a speculative neurology created by psychologists are scarcely original with the author. And her naïve faith that Ebbinghaus' nonsense syllables were really uniform and meaningless, while perhaps surprising in an exponent of *Gestalttheorie*, is shared with many other writers. There is, on the other hand, a pleasant absence of unguarded extrapolation from meager data and a praiseworthy abstinence from donning the long robe of Science which add no little to the dignity of the presentation.

All in all, the brief volume is a scholarly, but readable, affair which might well serve as corollary reading in many of the overroutinized elementary courses offered in this country today.

JOHN G. JENKINS.

University of Maryland.

ETHEL, G. Writing your novel. Portland: Scholastic Press, 1938. Pp. ix+273.

Artists and literary critics usually write about their subjects in terms that are vague and often supernatural. This essay, however, is written by a person who is well versed in the point of view and the terminology of contemporary objective psychology and who holds that literature must now stand consonant with modern science.

In scientific fashion the essay examines the novel as a phenomenon of human purposive behavior concerned with problems of adjustment. It proceeds, in stimulus-response terms, to examine character and its portrayal, setting, and action, and the technique of the novel in relation to its subject matter, purpose, and audience.

The consideration of absolute standards and judgments, art for art's sake, and romanticism strips the old arguments and postures of their confusion and glamour. They are laid bare in terms of their causes and consequences and are found as inadequate as illusion and wishful thinking. Art standards are the product of human behavior at a particular time in history and are, of course, relative human values. Because the artist is selective, and because his product has some purpose, it must reflect his partisanship. But perhaps he is partisan for good reasons. His argument is his novel. He writes because he wants to influence the thinking and action of people in order that their lives may be fuller and richer. The road to more happy adjustment is through the realism of truth and not through fairy tales.

Although the style of the book may perhaps be ponderous, the content and approach signalize the extension of the view of modern psychology into literature.

RALPH H. GUNDLACH.

University of Washington.

BOOKS RECEIVED

BRUNTZ, G. G. Allied propaganda and the collapse of the German Empire in 1918. Stanford Univ.: Stanford Univ. Press, 1938. Pp. xiii+246.

BURLOUD, A. Principes d'une psychologie des tendances. Paris: Félix Alcan, 108, Boulevard Saint-Germain, VI^e, 1938. Pp. 430.

BUROS, O. K. (Ed.) The 1938 mental measurements yearbook of the School of Education, Rutgers University. New Brunswick: Rutgers Univ. Press, 1938. Pp. xiv+415.

DUNBAR, H. F. Emotions and bodily changes: a survey of literature on psychosomatic interrelationships 1910–1933. (2nd ed.) New York: Columbia Univ. Press, 1938. Pp. xl+601.

FLETCHER, P. Life without fear. New York: Dutton, 1939. Pp. 111.

FRITZ, M. F. Collection and presentation of statistical data in psychology and education. New York: Prentice-Hall, 1939. Pp. vi+58.

GEORGE, W. H. The scientist in action: a scientific study of his methods. New York: Emerson Books, 1938. Pp. 354.

KELLER, F. J., & VITELES, M. S. Vocational guidance throughout the world: a comparative survey. New York: Norton, 1937. Pp. xiii+575.

ROBERTS, H. The troubled mind: a general account of the human mind, and its disorders and their remedies. (With chapters on the Insanities by M. N. Jackson.) New York: Dutton, 1939. Pp. iv+284.

SANDIFORD, P. Foundations of educational psychology: Nature's gifts to man. New York: Longmans, Green, 1938. Pp. xv+464.

THURSTONE, L. L. Primary mental abilities. Chicago: Univ. Chicago Press, 1938. Pp. 121.

NOTES AND NEWS

Dr. George Van Ness Dearborn, chief of the Department of Medical Psychology of the U. S. Veterans Administration, has died at the age of sixty-nine years.—*Science*.

Dr. John W. McGarvey, instructor of psychology at Mount Holyoke College, South Hadley, Massachusetts, died on December 26.

Dr. Edward H. Cameron, professor of education at the University of Illinois and director of the University summer session, died on December 20 at the Albert Billings Hospital, Chicago, at the age of 63.

Dr. Karl Bühler of Vienna has accepted a temporary position (until June, 1940) at St. Scholastica in Duluth, and will assume a teaching position at Texas this coming summer.

Doctor Norman Cameron, formerly associate in psychiatry in the Johns Hopkins Medical School and resident psychiatrist in the Johns Hopkins Hospital, has resigned to take charge of the laboratory for experimental psychology and psychopathology at the Payne Whitney Clinic, New York Hospital. He has been appointed associate professor of psychology in the Cornell Medical College and assistant attending psychiatrist in the New York Hospital.

Dr. Henry N. Peters, assistant professor of psychology at the University of Missouri, has been granted a leave of absence from February to September, 1939, in order that he may serve as director of classification in the Missouri State Penitentiary at Jefferson City, Missouri. Mr. Willis H. McCann, recently at Indiana University, has been appointed instructor in psychology for the current semester.

A meeting of members of the American Association for Applied Psychology who had subscribed to the incorporation of the Association, granted under the laws of the State of Indiana on December 14, 1938, ratified the revised by-laws, and officially recognized the election of officers and members of the Association. This meeting was held at Ohio State University on December 21, 1938. Affiliation with the American Association for the Advancement of Science was officially endorsed by the members of the A.A.A.P. who were present, and affiliation with the American Psychological Association was ratified at the same time.

A COURSE in the application of the Rorschach Method will be offered by Dr. S. J. Beck at Michael Reese Hospital, Chicago, Illinois, May 15–20, 1939 (the week following the annual convention of the American Psychiatric Association). Tentative plans are also being made for a similar 10-day course to be offered later in the summer, the dates being arranged to meet the convenience of prospective registrants. Information concerning these courses may be obtained from the Librarian, Michael Reese Hospital.

A NEW scientific periodical, *Psychosomatic Medicine,* started publication in January of this year. The journal will be published quarterly on a coöperative, nonprofit, nonsalary basis and will be sponsored by the National Research Council, Division of Anthropology and Psychology, Committee on Problems of Neurotic Behavior. The aim of the journal is to " encourage and bring together studies which make a contribution to the understanding of the organism as a whole, in somatic and psychic aspects." " Each number will include a review of relevant literature in the field of one or more of the medical and research specialties." In addition, *Monograph Supplements* will be published, and will include experimental data from longer studies. The Board of Editors invites submission of manuscripts for consideration for publication in these journals. Reprints of all articles published will be mailed to authors of those articles before distribution of the journal. It is expected that all authors will agree to the purchase of at least 200 preprints. Correspondence relative to the publication of articles should be addressed to Dr. Flanders Dunbar, Managing Editor, Academy of Medicine Building, Room 445, 2 East 103rd Street, New York City.

THE Public Information Section of the Works Progress Administration, 70 Columbus Avenue, New York City, has prepared a news release of facts revealed by the Psychological Index Project of the Administration of that city. These facts follow in summary:

World interest in psychological research, as measured by the number of articles published in scientific journals, continued at a high pitch through 1938. Articles published in all languages totaled 6693—an increase of more than 10% over 1937. Only one year in the post-War period—1931—topped this figure with 6792 articles.

An analysis of the distribution by languages discloses that maintenance of a high level of activity in English is the principal reason for continuance of the world total at close to peak figures. A very high percentage of the titles published in English is American. Titles in English reached a total of 4038 in 1938—an increase of more than 7% over 1937 and a new peak for the period since the War. Unusual features continue to be presented by the trend of publications in German. While the total there reached 1478 in 1938—an increase of more than 27% over 1933, it is still more than 44% below the post-War zenith of 2658 articles published in 1930. Titles in French, numbering 413, indicate a 9% increase over 1937. The analysis reveals that the rate of increase in all languages from year to year has been subsiding recently from the pace set during the decade of recovery following the War.

The WPA study is based, during the earlier part of the period under consideration, upon tabulations made from listings in the *Psychological Index* by Professor Samuel W. Fernberger of the University of Pennsylvania. The latter part of the period is based upon a similar count in the *Psychological Abstracts* made within the Psychological Index Project. Professor Albert T. Poffenberger of Columbia University is technical director and Harold C. Brown is supervisor of this project.

DR. C. M. LOUTTIT, Indiana University, Bloomington, Indiana, who is attempting to compile a directory of psychological associations in the United States, would appreciate having the secretaries of such associations, especially those of a local nature, send information about their organizations to him. The information desired includes name, purposes, officers, size of membership, and frequency and dates of meetings.

THE prize of $1000, which is awarded each year by the American Association for the Advancement of Science for an outstanding contribution to science, was awarded this year at the meeting of the Association in Richmond, Virginia, to Dr. Norman R. F. Maier, assistant professor of psychology in the University of Michigan, for his paper on "Experimentally Produced Neurotic Behavior in the Rat." In referring to Dr. Maier's experiments, the committee on award stated: "He has produced in rats behavior, the neurotic character of which seems to the committee, and to the critical audience which heard his paper, to be beyond doubt. This behavior was produced under conditions so controlled that cause and effect can be scientifically analyzed. The committee does not feel that the author's analysis of the phenomena is complete, nor does it believe that the small number of rats in which neurotic behavior was experimentally induced is sufficient for generalization or sweeping conclusions, and it gives credit to Dr. Maier for the conservatism he exhibited and for the scrupulous avoidance of applying his discoveries prematurely to the field in which they ultimately will be vastly significant—namely, neurotic behavior in human beings."—*Science,* 1939, **89**, No. 2301, 93.

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Psychological Bulletin

PSYCHOLOGY AND THE SCIENCE OF SCIENCE

BY S. S. STEVENS

Harvard University

The scientist has always been proud of his hard head and his tough mind. When William James (29) sat in judgment and divided the universe of temperaments into the tough- and tender-minded, the scientist knew where he belonged. He was happy to run with the goats, for he was an empiricist and he loved facts in all their crude variety. He was skeptical and cautious of word, and to 'isms' of all kinds he was peculiarly unresponsive. The tender-minded were the rationalists. They had faith in intuition and were awed by the power of the mind. It was their opinion that by taking thought they could discover absolute principles of truth answering to the criteria of coherence and consistency and that, armed with these principles, they could legislate the bounds of science. They were the sheep whose wool shone white under the light of reason. They were most numerous in departments of philosophy.

Undoubtedly these two types are still with us, but it is the purpose of this review neither to shear the sheep nor tame the goats. Instead, its purpose is simply to invite attention to some recent developments in what we might call the Philosophy of Science.

The tough-minded scientist has always known that he could screen his integrity against the seductive pipings of the rationalist by ignoring philosophy. The tender-minded philosopher, gifted with his superior dialectic, has usually despaired at the stubborn naïveté of the scientist and has determined to leave the unrefined fellow to grovel alone, while he, the philosopher, calmly demonstrated the impossibility of proving anything by induction. Suddenly, however, we find, on the one hand, a coterie of philosophers plying us with what, if it is not science, is certainly not the brand of stuff we have ordinarily pigeonholed as philosophy; and, on the other hand, we are beset by a host of scientists of all disciplines campaigning for what, if it is not philosophy, is surely not the science we are used to.

The philosopher, Benjamin (**3**), says of these scientists:

> "They begin with science, they talk about science, and they end with science, yet they do not conform at all to the tradition of scientific writing. . . Their repeated reference to philosophical issues tempts one to classify them with this group, yet the writings approach these problems in a new spirit and with a new method, which seem quite foreign to traditional philosophy."

And concerning the widespread groups of philosophers participating in this movement, Reichenbach (**51**) observes:

> "Though there is no philosophic system which unites these groups, there is a common property of ideas, principles, criticisms, and working methods. . . It is the intention of uniting both the empiricist conception of modern science and the formalistic conception of logic . . . which marks the working program of this philosophic movement."

So numerous and insistent are the words of those who have been seized by the spirit of this movement that they swell the pages of several new journals—journals whose subject matter defies simple classification.[1] There are articles by philosophers, mathematicians, and scientists. But it is more than a mere scrambling of the sheep and the goats. A common spirit animates most of these writings. The common theme, despite its fundamental simplicity, despite differences of interpretation by newborn enthusiasts, and despite the disparagement of misunderstanding, is probably to be esteemed as a truly great advance in the Philosophy of Science, or the Science of Philosophy.

Numerous phrasings of this central theme have been cast by authors interested in various aspects of it, but they all assert essentially that *science seeks to generate confirmable propositions by fitting a formal system of symbols (language, mathematics, logic) to empirical observations, and that the propositions of science have empirical significance only when their truth can be demonstrated by a set of concrete operations.* There are thus two separate realms of discourse:

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[1] Some representative journals are: *Erkenntnis,* begun in 1930; *Philosophy of Science,* begun in 1934; and *International Encyclopedia of Unified Science,* begun in 1938. The advisory boards of these last two publications read like a who's who in science and philosophy. It would be a passionate optimist, however, who would expect such a band of hardy individualists to be entirely of one mind. Many of them have not yet spoken.

the *formal* (or rational) and the *empirical.* It is the business of the philosopher to labor with the formal and discover and perfect the rules of the scientific language, and it is the business of the scientist to apply the formal symbolic model to the observable world in such a way that the concepts he generates will satisfy the rules of operational criticism.

Elementary as these notions may appear, the development of their implications has commanded the interest of both tough- and tender-minded. The movement has proved disastrous for metaphysics, challenging for logic, and salutary for science. Philosophers and scientists in essential agreement are astonishing enough, but here we have them pleading for a common method. In this strange harmony we are witnessing the birth of a new discipline: the Science of Science. It is a triumph for self-consciousness. The science-makers are asking themselves how they make science and are turning on that problem the powerful empirical weapons of science itself; while at the same time a tough-minded outcropping among the philosophers is carefully combing the metaphysics out of logic in order to investigate more easily the common linguistic structure of science. In this quest the philosophers, like the scientists, resort to empirical methods. Witness the spirit of philosophy as exemplified by Nagel (43):

> "It is difficult for me to take seriously the various attempts made historically by philosophers to legislate to the sciences just what they can and cannot investigate . . . on the basis of a deductive theory of mind and nature. . . Furthermore, it seems to me an integral character of skilled workmanship to insist upon the fact that no statement or proposal has any meaning apart from the methods which are or may be employed to establish or execute them."

In succeeding pages we shall see how operationism, beginning at one end in the laboratories of scientists, evolved an enterprise coördinate with that of Logical Positivism, Physicalism, and Scientific Empiricism which, beginning at the other end in the armchairs of philosophers, settled on the problem of the proper scientific use of logic. And we shall see how the natural issue of this mating came to make up the unifying principles of the Science of Science. We shall see how this movement concords with 'behavioristics,' which is a behavioristic psychology tuned up to keep pace with a fast-moving logical criticism. And finally, we shall see what the impact of this movement means for some specific problems in psychology, and what is indicated as the future rôle of psychology in this scheme.

OPERATIONISM

Ten years ago Professor Bridgman, the expert on high-pressure phenomena, wrote a book[2] called *The logic of modern physics* (11). It has been judged an excellent book, animated by the single idea that " in general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.*" This dictum stands forth in what many have found to be objectionable nakedness, but, throughout more than 200 well-stocked pages, Bridgman demonstrates what he means by analyzing the operational meaning of the basic concepts of physics. There is nothing rationally *a priori* in his method (at least he honestly *tries* to exclude metaphysics). His introductory confession is: " The material of this essay is largely obtained by observation of the actual currents of opinion in physics." In this empirical spirit he observes the behavior of his colleagues and finds that what is considered an *explanation* " consists in reducing a situation to elements with which we are so familiar that we accept them as a matter of course, so that our curiosity rests." The reduction of the " situation " is made in terms of operations, but do we thereby arrive at exact and certain knowledge? No. " We never have perfectly clean-cut knowledge of anything, but all our experience is surrounded by a twilight zone, a penumbra of uncertainty, into which we have not yet penetrated," and consequently " no empirical science can ever make exact statements." The degree to which any of the laws of science wear the penumbrous halo can be told only by inspecting the operations which the laws are intended to generalize.

Bridgman's book is rich in example but poor in precept. That its author has occasionally been misunderstood has perhaps been due largely to this fact. The book gives numerous examples of operational method without prescribing explicitly what operational method is; it talks of " operations " without giving an explicit definition of the term; and it discourses on natural laws without pointing out how we get from particular operations to generalizations. In short, it is a thoroughly inductive enterprise, and the reader is often allowed to make the induction by himself. Nevertheless, the spirit of the book is unmistakable and its message is simple and powerful.

Philosophers rose to protest, or sometimes to defend, the notion of " operational meaning " because it assures the automatic elimination of even the choicest propositions of metaphysics: " If a specific question has meaning," says Bridgman, " it must be possible to find operations by which an answer may be given to it." No operations, no meaning! And so, as we have said, philosophers, and others, rose to protest. Finally, the pressure pushed Bridgman temporarily from his Harvard laboratory and on to the lecture platform at Princeton where he spoke what became another book: *The nature of physical theory* (13).

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[2] For additional comments on some of the books and papers cited in this review see the " Bibliography."

To say that this second book pleased all who were disciples of the first book is perhaps not quite true. The author had been able to say in his first book that fortunately he would " be able to get along with a more or less naïve attitude toward " psychology and epistemology, but in his second work he boldly lays hold on thought, language, and experience. Bridgman's discussion of these concepts was what the world had been waiting for, but once out of the well-charted sea of physics and adrift in epistemology, the author's bark, if we are to believe the critics, appears to have lost its rudder. One cannot avoid the impression that criticism of this second book has been of unmerited severity, but perhaps severe criticism is what must be expected by a man who challenges us with issues as vital as those proposed by Professor Bridgman. Objection has been made to such statements as: " In the last analysis science is only my private science." " What," asks editor Malisoff (34), " can an operationist mean by a ' last analysis ' ? " Bridgman says his purpose in sailing the epistemological waters is " to map out the possibilities and limitations of the human mind in dealing with the problems presented to it." " Our complaint," criticizes A. F. Bentley (6), " is not that he makes this inquiry, but that in it he employs all the bad devices he had ejected from physics." Some of his devices are assertions crutched on such terms as " essentially," " absolutely," and " intuitively." Nevertheless, Bridgman's critics agree that his discernment in physics remains as fine as ever—he is still simple and hardheaded. In physics he is an operationist, and it is in physics that we should judge him, if we are to presume to do so.

Just as Bridgman had set out to apply and make explicit the principles by which Einstein shattered the physicist's notion of the absolute, so did others seize upon the opportunity to try out these principles in other fields. Psychologists, long self-conscious of their own self-consciousness, were particularly alert to this budding self-inspection on the part of the modern masters of physics. If the physicists could examine the methods of their science-making and evolve helpful principles, perhaps psychologists could do likewise.

Such, at least, was the attitude of those who were happy to confess blindness to any fundamental dichotomy between the methods of psychology and physics.

OPERATIONISM IN PSYCHOLOGY

But psychology is more difficult than physics—at least psychologists often find it easier to get themselves into a mess in their field than physicists do in theirs. Of course, when the physicist strays into psychology, the result is apt to restore the psychologist's ego-level, but if the physicist fumbles it only serves to show that when doing open-field running among psychological concepts the critic must hold the ball more tightly. In view of the difficulty of keeping a grip on the operational principle, it is not surprising to find evidence of dissension among the psychological apologists (Professor Bills (7) calls them "apostles"). In spite of much scattered writing, the case for operationism in psychology has perhaps never been adequately briefed, but a few of its consequences have been made explicit, and some interesting applications have appeared.

We all remember Tolman's *Purposive behavior in animals and men* (60). Whatever dismay we may have felt at the superabundance of his glossary, the fact remains that in coining the words of his new language he appealed most directly and explicitly to experimental operations. The book is a monument in the methodology of definition. In much the same spirit, Professor Tolman has more recently prepared for us "an operational analysis of 'demands'" (61). In his own field of expertness we find McGeoch making a critical inquiry into the possibility of "learning as an operationally defined concept" (37). Boring treats of "temporal perception and operationism" (10) in a short, poignant demonstration of how a classical problem turns out to be specious when its defining operations are made explicit. Seashore and Katz propose to bring order to the chaotic discipline of abnormal psychology by "an operational definition and classification of mental mechanisms" (54). Lundberg, the sociologist, would do the same for the social sciences by replacing spineless intuitionism by "quantitative methods in social psychology" (33), the foundation for which would be concepts operationally defined. And finally, Kantor examines "the operational principle in the physical and psychological sciences" (30) and concludes that the principle, properly enlarged, can be employed to the psychologists' advantage. Now, these are not all of those who have taken notice of Bridgman's proposals. Nor do all these commentators see eye to eye with Bridgman or with each other regarding certain fundamentals. Furthermore, it is becoming alarmingly obvious that the phrases 'operationally defined' and 'operationally sound' are acquiring the sort of positive valence which leads to their being bandied about in indiscriminate fashion by writers who suppose that they can meet the operational test by announcing good intentions. Operationism is being threatened by its friends—largely, perhaps, because of the inherent difficulty of making a rigorous formulation of it.

What, then, are we to understand by operationism? All that any one man can do is to present his own version, and this I did in a series of articles in 1935 and 1936 (**56**, **57**, **58**). There are some points there which invite revision, but, in general, the sins appear to be those of omission. The statement there needs expansion, but obviously this review is not the place for it. A résumé is more in order.

First, however, it must be emphasized again that the development of operational principles is properly an empirical undertaking. What do the science-makers do? What methodology has the maximum survival value? When do propositions have empirical validity? In short, operational principles are induced generalizations rather than *a priori* fiats. They are therefore subject to the usual hazards and uncertainty of inductive propositions. This empirical aspect of operational criticism has never been sufficiently stressed, and it is not surprising that operationists have sometimes been regarded as self-appointed legislators who try to prescribe rather than discover.

These, then, are some of the generalizations which I propose as verifiable:

1. Science, as we find it, is a set of empirical propositions agreed upon by members of society. This agreement may be always in a state of flux, but persistent disagreement leads eventually to rejection. Bridgman does not agree to this social criterion of knowledge and it was against this notion that he aimed a part of his Princeton lectures (**13**). We must ask him, however, to produce the negative case. A physical law to which only Bridgman agreed would not be a part of physics—not, at least, until he won converts, and then there would be agreement.

2. Only those propositions based upon operations which are public and repeatable are admitted to the body of science. Not even

psychology knows anything about private experience, because an operation for penetrating privacy is self-contradictory.

3. What becomes acceptable psychology accrues only when all observations, including those which a psychologist makes upon himself, are treated as though made upon 'the other one.' Thus, we make explicit the distinction between the experimenter and the thing observed. This distinction is obvious in physics; in psychology it is equally valid.

4. Although a particular experimenter may himself become the object of study by another experimenter, and he in turn by still another, at some stage of such a regress an independent experimenter *must be* (*i.e.* is always) assumed. The recognition of this 'experimenter-regress' unravels many knots in psychology.

5. A term denotes something only when there are concrete criteria for its applicability; and a proposition has empirical meaning only when the criteria of its truth or falsity consist of concrete operations which can be performed upon demand.

6. When we attempt to reduce complex operations to simpler and simpler ones, we find in the end that discrimination, or differential response, is the fundamental operation. Discrimination is prerequisite even to the operation of denoting or 'pointing to,' because whenever two people reduce their complex operations for the purpose of reaching agreement or understanding, they find that unless they can each discriminate the same simple objects or read the same scales they still will not agree. Agreement is usually reached in practice before these most elementary operations are appealed to.

7. There are two types of propositions: *formal* and *empirical*. The formal propositions are arrays of symbols without empirical reference. They are language, mathematics, and logic *as such*. Empirical propositions are those in which these arrays of symbols have been identified with observable events. Sometimes the two types of propositions intermingle and trouble results. For avoiding the obscurity of pseudo-problems this distinction between the formal, syntactical model (symbols) and the operations for which it is made to stand is of prime importance. Hypotheses, for example, can be only formal statements—operationally empty—until they are demonstrated (see Appendix II). Within the formal realm we speak sometimes of mathematical operations, but here we mean the manipulation of

symbols carried out according to certain conventional rules. These are not the operations of operationism.

Although we shall have more to say later about the contrast between the formal and the empirical, at this point we might do well to see how history occasionally sets them off from one another and thereby emphasizes their distinctive natures. Historically, the algebra of complex numbers (numbers of the form $x+iy$, where x and y are real numbers and i is the square root of -1) was developed from the purest of purely mathematical motives. The rules for the manipulation of these numbers (their addition, multiplication, division, etc.) were worked out in conformity with the conventional laws of ordinary algebra and interesting relations were discovered. Gauss, for example, set a landmark in algebra by proving that every algebraic equation in 1 unknown has a root and that all roots of such equations are complex numbers (see Bell, 2, p. 232). In Gauss's time these numbers were simply abstract symbols which could be combined according to the rules of the game we call algebra. They proved nothing about the empirical world or about science: they constituted, as they still do, a purely *formal* system. Then, with the advent of alternating electric currents, came also the need for a simple, effective 'model' to represent electric circuits; and the electrical engineers discovered that if they let x stand for resistance, iy for inductive reactance, and $-iy$ for capacitative reactance, they could manipulate these symbols according to the rules of complex algebra and obtain new combinations of the symbols which they could then identify with some measurable aspect of an electric circuit. In other words, this formal system was found useful as a model, and out of its utility has grown the modern intricate theory of alternating currents. Therefore, when we can identify these complex numbers with various aspects of a circuit, we can say that the propositions containing these symbols are *empirical* propositions, testable by concrete operations.

These seven bald assertions about operationism are perhaps too brief to be convincing, but they may recommend the fuller development in the three papers already referred to. In the meantime we might profit by considering what operationism is not—still, of course, in only one man's opinion. Misunderstandings have been numerous and many of them could have been headed off had someone signaled what is nonoperational. Let us, then, look at a few of operationism's contrasts.

WHAT OPERATIONISM IS NOT

1. It is obviously not a new school of psychology. Rosenzweig (52) presented an admirable argument to show that the schools of psychology are really more complementary than antagonistic, but he was worried about operationism. He should stop worrying.

2. It is not a set of rules telling how to be a bright and original scientist. It does not even tell how experiments should be carried out. It has no precepts. At the risk of breeding disappointment we must say of operationism, as James (29) said of pragmatism, that, "at the outset, at least, it stands for no particular results. It has no dogmas, and no doctrines save its method." Furthermore, its method is one which is applied *after* the scientific proposition has been made: it provides criteria for determining whether what *has been* said is empirically meaningful. In short, it tests inventions, but does not tell how to invent.

3. It is not opposed to hypotheses, theories, or speculation. It seeks merely to discover criteria by which these things may be detected and labeled. It is not opposed to poetry, art, or religion. It wants only to know the difference between these things and science. It wants to know under what conditions the consorting of science with metaphysics breeds pseudo problems. Scientists as people may be opposed to pseudo problems, but operationism's business, as a principle of criticism, is to discover them.

4. It is not a guarantee of agreement as to tastes or theories, but it points out how agreement as to facts is achieved by men capable of making the same fundamental discriminations. Operationism wants, most of all, to discover the bases of such agreement. What are the procedures which compel agreement among those engaged in open-minded pursuit of science? As to compelling agreement on tastes—that is probably a job in applied eugenics.

5. It is not positivism. The blemish on positivism was that in its reaction against rational metaphysics it pretended to base *everything* in science on experience. Operationism, however, acknowledges the rôle of the rational methods of mathematics and logic—formal disciplines which do not appeal to experience for verification, but only to conventions. Science uses these formal systems as models for representing its data. To deny them is to cure the disease by burying the patient.

When it is a matter of the significance of *empirical* rather than of *formal* propositions, needless to say, operationism adopts an uncompromising positivistic attitude.

6. It is not behaviorism. Like positivism, behaviorism erred in denying too much. Operationism does not deny images, for example, but asks: What is the operational definition of the term 'image'? Of course there are different behaviorisms, and some of the renovated brands are truly operational. Tolman (**62**) has a variety which he dubs explicitly "operational behaviorism"—and perhaps it is. It is certain that the behavioristic emphasis has served capably in blasting a path through subjectivity, and without this path an objective Science of Science could not march.

7. It is not monism. It asks only whether any operational meaning can be given the proposition that there is but one irreducible substance or attribute. Can the truth or falsity of this proposition be put to experimental test? If not, we face a pseudo problem.

8. It is not dualism. Here again the problems raised are pseudo problems, because the propositions are not testable. As Bills (**7**) so aptly says, "Parallelism would automatically reduce to a double-aspect formula, because where two sets of defining operations coincide perfectly they become identical operationally." Of course there can be no quarrel, except on the grounds of utility, with any arbitrary dividing or classifying of facts, but pseudo problems can be avoided only provided we remember that these classes are arbitrary.

The division of concepts into the categories of subjective and objective is justifiable—if at all—only on pragmatic grounds, and only *provided* both types of concept answer the operational test. Bills believes that "mentalistic" concepts like percept, image, and idea can be operationally defined. So do I. Kantor, however, is disturbed. He detects dualism. But Bills "cannot agree with Kantor that there is any necessary dualism implied in Stevens' position." Neither can Stevens. If we admit to our store of empirical science only those concepts which are operationally founded, can we not classify them according to our purposes?

Kantor (**30**) would appear to supplant dualism with a kind of realism. Now, realism is a metaphysical doctrine, and perhaps Kantor did not intend a realism. Nevertheless, he appears to defend the proposition: Nature is not the same as our knowledge of nature. Operationism must here again pose its perhaps tiresome, but necessary, question: Can any operations be formulated which will either prove or disprove this proposition? If not, it is operationally meaningless, however much 'emotional meaning' it may pack.

9. Finally, operationism is not pluralism. It should be apparent by now that operationism is not consonant with any 'ism' which asserts something about the ultimate nature of reality.

THE PROBLEM OF GENERALITY

There is one more criticism we must take seriously before we continue. It has been urged that operationism reduces to a vicious particularism; that there is no provision for generalization; that instead of unification in science a strict servility to the operational principle nourishes an ever-expanding multiplicity of concepts. Here is what the critics say:

Margenau, in "Causality in modern physics" (**35**), which he addressed to the philosophers, states that operationism "cannot be tolerated as a general directive. For, in the first place, it would, if carried to its consequences, dissolve the world into an unmanageable variety of discrete concepts without logical coherence."

Lindsay, in "A critique of operationalism in physics" (**31**), says: ". . . . logically the operational method . . . . implies that each concept is tied to a definite operation."

Lindsay and Margenau together, in their book, *Foundations of physics* (**32**)—a book which has brought them merited high praise, state: "On the basis of purely operational definitions, all concepts are strictly empirical and isolated" (p. 412).

Bills, in his excellent address on "Changing views of psychology as science" (**7**), says: "One of the ideals of scientific concept-makers is to reduce all concepts to a few fundamental ones. . . Yet this is not, by any means, the likely outcome of operationally defined concepts. . . For there is no universal set of operations."

Waters and Pennington, in their careful criticism of "Operationism in psychology" (**63**), assert: "The fact that the concept, for Bridgman, is *synonymous* with a corresponding set of operations cannot be overemphasized. The slightest change in any aspect of a set of operations would mean, therefore, a new concept and would demand, likewise, a new symbol for its designation. A multiplicity of concepts could scarcely be avoided."

Since Bentley (the critic, not the psychologist), in his flashy tirade on "Physicists and fairies" (**6**), has a point to make here, we will let him speak first. He refers to Lindsay and Margenau when he says: " By distorting Bridgman grossly enough, either man can, of course, readily destroy what he has distorted. Both men distort alike; first by insisting ' operations ' must be all hands and no mind; second by alleging that no operation in this world can have anything to do with any other operation, not even with its own repetitions of itself."

Whether there is distortion or not, the fact that so many have pounced on this supposed snare in operationism means that the rules and procedure for generalizing from operations must sometime be made explicit. These rules obviously can be stated, because science does generalize, and operationism seeks only to discover how scientists do what they do.

The process of generalization proceeds on the basis of the notion of classes. All objects or events satisfying certain criteria we call members of a class and to that class we assign a name or symbol. Common nouns originate in precisely this fashion, and it is apparent at once that no empirical proposition is ever without some element of generality. Classification can proceed only when we have criteria defining the conditions for class-inclusion, and these criteria are essentially operational tests. Thus the statement, " Dobbin is a horse," asserts that Dobbin is a member of a class. This proposition is empirically meaningful only provided its truth or falsity can be demonstrated by concrete procedures. Does Dobbin satisfy the criteria of the class, *horse?* If he is a certain size and shape, is covered with hair, feeds on oats and hay, etc., we are happy to acknowledge him as a full-fledged horse. But how do we know he meets our tests? Here we resort to that fundamental operation we have already called discrimination. If we can discriminate crucial differences between Dobbin and other animals we have named horses, we reject Dobbin as something not horse. In other words, we ' correlate ' our discriminations—those made on Dobbin with those made on other objects—and the ' goodness ' of the correlation determines where we shall classify the beast.

It may be objected that we can always tell Dobbin from other horses, *i.e.* discriminate differences, but we still would resent the suggestion that he is not a horse. The answer is that a certain latitude is always allowed—we seldom resort to j.n.d.'s in a case like this—and the amount of the latitude determines the precision of the concept. As Bridgman has insisted, no concept is without its halo of uncertainty, its penumbra. No empirical class is ever watertight; we can always plague the taxonomist with the borderline case.

On the basis of elementary discriminations, then, we make our first rudimentary classes and in doing so we have made the first step toward generalization. From there we advance to form classes of classes [3] and to discover the relations between classes—always, at the empirical level, in keeping with operational criteria. Occasionally we find that from a certain point of view two classes satisfy the same criteria, or are related by a simple law, so that we are enabled to combine them into a more inclusive class under a more generic tag. Nevertheless, in all of these classifications and combinations the same simple rule is followed: We combine operations when they satisfy the criteria of a class; and the concept of that class is defined by the operations which determine inclusion within the class.

The matter can be illustrated by referring again to that example which appears to have been the jumping-off place for the critics: the concept of length. Bridgman's argument is that we measure the length of the table and the distance to a star by two different sets of operations and we have, therefore, two different concepts of length. True enough. And Bridgman proceeds thence to show that when dealing with very large distances or very minute ones, or with distances where velocities are involved, we do well to keep in mind the differences in our defining operations. However, in his concern for the perils of promiscuous class-matings he forgot to tell us when combining is legitimate. Length measured with a rod is different from length measured with a transit, but under certain statable conditions we can muster operations to determine the relation of these two sets of measurements, and, if they meet the proper criteria, we combine them to form a larger class defining length. Of course, if we had no operations for comparing the two lengths, we should have to veto their combination. In short, then, we can and do generalize the concept length, but we do it with operational sanction.

THE PHILOSOPHICAL MOVEMENT

Just ten years ago, the year Bridgman published his *Logic of modern physics,* there appeared in Vienna a company of scholars bound together by mutual admiration and a common *Weltauffassung*—a scientific philosophy. Their discussions under the leadership of Professor Schlick accomplished a unitary enthusiasm which came to concrete form in the organization of *Der Wiener Kreis.*[4] The avowed intention of this "Circle" was to replace philosophy by the systematic investigation of the logic of science which, for Carnap, is "nothing other than the logical syntax of the language of science." There are but two kinds of acceptable propositions: *formal* and *empirical.* Formal propositions concern syntax. They state the rules and procedure for combining words or symbols and have no empirical reference. Empirical propositions are assertions about the observable world and their truth or falsity can be tested by means of observational procedures. Since metaphysics consists of statements not susceptible to empirical test, it is either an array of syntactical (formal) sentences or else it is technical nonsense. Mostly it is nonsense. Philosophy must be purged of it; and, once purged, it becomes the business of philosophy, says the Circle, to investigate the rules of the language we use in formulating our scientific propositions. The goal of such philosophical research is to provide a secure foundation for the sciences.

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[3] This *empirical* process of forming classes of classes should not be confused with the *logic* of classes, in which the provision for an infinite hierarchy of classes led to the antinomies discovered by Russell. The empirical process has no *necessary* relation to a formal system of logic.

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[4] Some of the members of the Vienna Circle follow:

Moritz Schlick (1882–1936) fathered the group. Under his professorial paternalism the Circle met, discussed, and found its unity. (Schlick's unfortunate death, at the hand of a crazed student, occurred as he was climbing the steps of the lecture hall.)

Otto Neurath (b. 1882) contributed his own brand of enthusiastic originality. His spirited support of radical new theses provided important inspiration. Neurath coined the designations "Physicalism" and "Unity of Science."

Rudolph Carnap (b. 1891) labored with the problem of syntax—the logical rules of language. His energetic attack on the problem of the actual construction of a fundamental syntax for the "physical" language has created a whole new field of inquiry.

Philipp Frank (b. 1884), a theoretical physicist, applied the new theory of knowledge to the problems of physics.

Hans Hahn (1879–1934), a mathematician, investigated the foundations of mathematics and exact science in the light of the scientific *Weltauffassung* of the Circle.

Friedrich Waismann distinguished himself with an investigation of the logical foundations of mathematical thinking.

In addition to these members of the Vienna Circle there were other groups whose scientific philosophy was so similar as to be scarcely distinguishable. In fact, one of the impressive aspects of this recent philosophical movement is the manner in which a common *Weltauffassung* appeared almost simultaneously among widely scattered groups of scientists, mathematicians, and philosophers. There was the Warsaw Circle, which boasted such able logicians as Tarski (b. 1901) and Lukasiewicz (b. 1878). At Berlin, prior to the recent cultural eclipse, there was another Circle whose outstanding advocate was Reichenbach (b. 1891). Logicians Russell (b. 1872) and Frege (1848–1925) fall into the same tradition, and in America C. W. Morris (b. 1901) is perhaps the best known expositor of the common program. For a more complete listing of names, see Neurath's "Historische Anmerkungen" (**44**).

This movement was not, of course, without its antecedents. Its most immediate point of departure was the famous *Tractatus logico-philosophicus* (**65**) by Russell's pupil, Ludwig Wittgenstein. The "Tractatus" exhibited the close connection between philosophy and syntax; it made clear the *formal* nature of logic and showed that the rules and proofs of syntax should have no reference to the meaning (empirical designation) of symbols; and it showed that the sentences of metaphysics are pseudo propositions. But the roots of these notions can be traced even back beyond Wittgenstein. All who, like the positivists, struck out at metaphysics; all who, like Kant, sought to conciliate analytic (formal) methods with the synthetic (empirical); and all who, like the British empiricists, assaulted philosophy with logical weapons have something in common with the Vienna Circle. Hume, in particular, except when he was assuming the existence of a transempirical world, caught the spirit. He winds up his "Enquiries concerning human understanding" (**28**) with this counsel:

> "If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, *Does it contain any abstract reasoning concerning quantity or number* [formal questions]? No. *Does it contain any experimental reasoning concerning matter of fact and existence* [empirical questions]? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion."

A philosophy as distinctive as that of the Vienna Circle must inevitably become an 'ism,' and its disciples, Blumberg and Feigl (**8**), lost no time in introducing the Circle's program to American scholars under the title of "Logical Positivism." A. F. Bentley (**5**) promptly raised the question as to whether Logical Positivism is either logical or positive, but in spite of some obvious disadvantages, the name is not entirely unreasonable. Bentley, as his readers know, loves a *bon mot* and has a low threshold for alarm—he is aroused to criticism easily but not unpleasantly. The name Logical Positivism quite properly suggests the union of the formal and the empirical—a union which, in a well-ordered scientific household, is possible and legitimate.

Logical Positivism proposes to tell us how such a household should be run. A certain division of labor is required. The scientist, in his special field, continues to investigate the empirical relations among the variables he has at hand and these relations he represents by some form of symbolic language. The philosopher complements the scientist by probing the nature and the rules of this symbolic language. Statements about the empirical domain are called object-sentences; statements about language-forms are syntactical sentences. In any special science, such as psychology, both types of sentences frequently occur, because the psychologist must tell us not only about his facts, but also how he intends to use his words and symbols—he must provide his own definitions (see Appendix I). The philosopher, on the other hand, can point out the logical implications of the psychologist's language and help him guard against the vicious combinations of the two types of sentences which lead to pseudo propositions.

Under this program it is not, however, the task of the philosopher to legislate for science. Science can use any logic it finds useful. Carnap (22), at this point, proposes a Principle of Tolerance to allay our fears: "It is not our business," he says, "to set up prohibitions, but to arrive at conventions." "*In logic,*" he continues, "*there are no morals.* Everyone is at liberty to build up his own logic, i.e., his own form of language, as he wishes. All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules instead of philosophical arguments." Consequently, he who sets out to scrutinize the logic of science must renounce the proud claim that his philosophy sits enthroned above the special sciences. He works in the same field as the specialist, only with a different emphasis. He ponders the logical, formal, syntactical connections. He studies rules which are basically nothing other than conventions and matters of free choice. Hence the labors of the philosopher in that which is his only legitimate domain, the logic of science, are bound to be barren unless they are pursued in close coöperation with the special sciences.

Logical Positivism, then, seeks (1) to clarify the language of science and (2) to investigate the conditions under which empirical propositions are meaningful. The language of science (including syntax, logic, and mathematics) consists of arrays of words or symbols which we assemble according to certain rules. The analytic propositions of syntax and mathematics are absolutely necessary and certain, once the rules of the game have been laid down. These propositions neither tell us anything about the empirical world, nor can they be confuted by experience. They can no more be proved 'true' than can the conventional rules of the game of chess (see below). They simply record our determination to use words and symbols in a certain fashion.

Mathematics, under this view, is a completely rational and deductive system and nothing is contained in one formula which is not implicit in all formulas. This, to many, is a fearful thought. Poincaré (**47**) voiced his apprehension by asking: " If all the propositions it enunciates can be deduced one from the other by the rules of formal logic, why is not mathematics reduced to an immense tautology? . . . Shall we admit that the theorems which fill so many volumes are nothing but devious ways of saying that A is A? " The answer appears to be that regardless of how inventive mathematical discoveries may appear to be, they contain nothing not already implicit in the fundamental postulates of the system. The outcome of our symbol-juggling surprises and delights us and fills us with the illusion of discovery, simply because of the limitations of our minds. A man of sufficient intellect would disdain the use of logic and mathematics, for he would see at a glance all that his postulates and definitions implied. He would be aware of all possible discoveries under the rules. The rest of us, however, must continue to do our mathematics stepwise, proceeding from one tautological transformation to the next, and being surprised at the result.

The second aim of Logical Positivism—to discover the conditions of empirical meaning—leads to the notion that an object-sentence is one which is verifiable by means of some concrete procedure. At this point operationism and Logical Positivism are essentially indistinguishable and we shall say no more about them, except to note an error.

This is an error which the Logical Positivists themselves have acknowledged and corrected (cf. Carnap, **21**, p. 11), but since the slip was made in what is commonly regarded as psychological territory, we had best have a look at it. The Vienna Circle committed the all too common fallacy: It claimed to find a difference between *knowledge* and *immediate experience* (see Blumberg and Feigl, **8**). Knowledge is communicable, but the immediately given is private and noncommunicable. This from the mouth of a Logical Positivist! Indeed, by all the rules they have proposed, this sentence is not a testable proposition, for how shall we demonstrate the existence of the noncommunicable? But, as already indicated, the Logical Positivists have not been stubborn about insisting that it makes sense to talk of the private content of immediate experience as being different from the discriminable and reportable relations between experiences. Their past lapse in this regard is interesting only because it shows how easy it is for even the well-intentioned to talk nonsense when they invade this field of psychology. In " The operational definition of psychological concepts " (57) I have tried to demonstrate that an empirical (operational) definition of immediate experience is possible provided we note precisely what its advocates do when we ask them to indicate an example of it. Almost invariably they point to a situation involving an elementary discrimination such as: " I see red." Elementary discriminations, then, are what is meant by the immediately given, and discriminatory reactions, of course, are public and communicable.

PHYSICALISM

As thoroughgoing empiricists the Logical Positivists hold that all meaningful scientific propositions are derived from experience. More precisely, all such propositions are reducible to *protocol-sentences*—sentences relating to the simplest elements of experience. This notion, I take it, is equivalent to the operationist's view that complex propositions are shown to be meaningful when they can be reduced to simpler propositions for which there are operational tests. The simplest propositions of all would be those relating to elementary discriminations. Now, if all scientific propositions are reducible in this fashion, including propositions expressed in what is called *physical language,* it must follow that *all* propositions are translatable into the physical language—a language similar to that of contemporary physics. This is the thesis of Physicalism.[5]

Physicalism was christened by Neurath (cf. 45). Contrary to what the name suggests, it is not a metaphysical doctrine asserting that everything is physical, for such a proposition can have no testable meaning. It is, on the other hand, a thesis relating to language: The physical language is a universal language of science and the individual languages used in any subdomain of science can be equipollently translated into the physical language. Innocent as this assertion about language may appear, it is charged with far-reaching implications for psychology. In fact, the examples used to illustrate Physicalism make it appear that the doctrine was aimed directly against psychology—at least against the kind peddled by philosophers.

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[5] This is a somewhat oversimplified statement of Physicalism. Furthermore, Carnap (21) has recently introduced extensive qualifications and changes into the original views of the Vienna Circle regarding the relation of the various 'languages' of science. His reasons for preferring the physical to the psychological language (pp. 9 ff.) do not appear to me to be binding, especially if the psychological language is made operational. If that is done, the choice becomes one based on convention or convenience. We could express all physics in psychological language, but that would be more traumatic to tradition than if we were to express all psychology in the physical language. The name Physicalism justifiably appeals to many as an unhappy designation, because it arouses prejudices by suggesting the primacy of a materialistic physics.

Physicalism makes it clear that the traditional but somewhat antiquated problem of psychophysical dualism is exclusively a problem of syntax. Using the common 'material mode' of speech we might say: To every psychical state there is a corresponding physical state of the body and the two are lawfully connected. Couched in this form, such a sentence is a veritable gold mine for pseudo problems. Physicalism would throttle these problems by saying: All sentences purporting to deal with psychical states are translatable into sentences in the physical language. Two distinctly separate languages to describe physics and psychology are therefore not necessary. And in this assertion we have Physicalism's denial of metaphysical dualism. It is the Logical Positivist's way of saying that psychology must be operational and behavioristic.

The philosopher, Hempel (27), calls this kind of psychology *logical behaviorism.* It differs from the primitive American stamp in that it does not prescribe that research shall be limited to stimulus-response connections. It is not, properly speaking, a theory *about psychology* at all, but only a logical theory about psychological sentences. The psychologist may study anything he pleases, but any verifiable psychological proposition he may utter is equivalent to some proposition in the physical language. An operationist would certainly agree to this notion. In fact, an operationist would point out that this view is correlative with his own dictum that any meaningful psychological proposition, even though it pertains to a toothache, is reducible to public, concrete operations.

THE UNITY OF SCIENCE

How we get from Physicalism to the thesis of the *unity of science* is obvious indeed. If every sentence can be translated into the physical language, then this language is an all-inclusive language—a universal language of science. And if the esoteric jargons of all the separate sciences can, upon demand, be reduced to a single coherent language, then all science possesses a fundamental logical unity.

This idea of a unified basis for science, introduced into the Vienna Circle by the imaginative originality of Neurath, has launched a whole new movement in scientific philosophy. The newly-begun *International Encyclopedia of Unified Science* is tangible testimony to the vigor and seriousness of the enterprise.[6] Annual congresses provide a forum where the thesis is developed (Fifth Annual Congress to be held at Harvard University, September 5–10, 1939); and out of this intellectual ferment there is emerging a substantial basis for an empirical and universal Science of Science. But before we inspect this newest of sciences—one which is obviously still warm in the womb of its philosophy-mother—let us look backward a few centuries.

How many men, since ancient Thales proposed that all is water, have dreamed the dream of a universal science is beyond a guess. The dream has taken many forms—mostly impracticable—for the history of science is a story of diversification and specialization proceeding almost geometrically with time. If there is unity in so much arborescence, where are we to find it? Certainly not in subject matter where differentiation is the rule. Perhaps, then, in method and logic.

In 1666 the twenty-year-old Leibnitz (**2**) dreamed his own dream about the unity of science and recorded it in *De Arte Combinatoria.* He himself called it a schoolboy's essay, but in it he proposed to create *"a general method in which all truths of reason would be reduced to a kind of calculation. At the same time this would be a sort of universal language or script, but infinitely different from those*

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[6] Neurath (**46**) describes *unified science* as *encyclopedic integration.* The new "Encyclopedia" is to be constructed like an onion. The heart of the onion will be two introductory volumes consisting of twenty pamphlets, and in these volumes will be laid the foundations for a logical unity which will make possible future integration of scientific disciplines. The first layer of the onion enclosing the heart will be a series of volumes to deal with problems of systematization in special sciences, including logic, mathematics, the theory of signs, history of science, classification of the sciences, and the educational implications of the scientific attitude. Still outer layers will concern even more specialized problems. The encyclopedia will not be an alphabetical dictionary and its creators hope, quite piously, that it will not become a mausoleum but remain a living intellectual force.

At the present writing only three numbers (1, 2, and 5) of the "Encyclopedia" have appeared, but it is already clear that, although there is great community among the contributors, detailed unanimity is absent. As to the problem of unity in science, for example, Carnap finds as yet *no unity of laws* in science, but only *unity of language*; Lenzen finds a basis for unity in the fact that all science starts from experience; Neurath would get his unity by means of *encyclopedic integration*; Russel says the unity is essentially one of method; and Dewey hopes for unity by promulgating what he calls the scientific attitude.

projected hitherto; for the symbols and even the words in it would direct the reason; and errors, except those of fact, would be mere mistakes in calculation." How long would it take to create this logistic? Leibnitz thought a few chosen men could turn the trick within five years. But chosen men were not at hand and two centuries passed before the creation of a universal symbolic logic was even begun. Almost another century of labor has been needed to lay a foundation in logic and syntax so tangible that many men together could vision the unity of science.

Leibnitz though, if any single man, was father to the idea. He hoped for a universal logicalization of human thinking by means of a general calculus and a general terminology. He conceived a formal discipline to include a theory and art of forming signs to represent ideas and a general calculus giving a universal formal method of drawing consequences from the signs. Then, if two men were to find themselves in disagreement as to anything except matters of observation, they would settle their argument by calculating the right answer. Leibnitz' inspiration is perhaps not without its utopian aspect, but it cannot be denied that the modern logic of science has made progress towards Leibnitz' goal.

Perhaps our progress has not always been of the sort that would have delighted the boy of twenty, for metaphysics was no triviality in 1666. Today, however, it is clear that the unhappy symphonies of pseudo propositions that are metaphysics have all too frequently thwarted our efforts at clarification. Logical analysis has unmasked metaphysics; at least that is one of the boasted achievements of the recent philosophical movement. Opinion will probably never be unanimous on this issue, but disclosure of the empirically meaningless aspects of metaphysics is intimately bound to the other advances claimed by the Logical Positivists. By way of review at this point, these are some of the achievements of the modern movement:

1. It has been demonstrated that a unified language of science is possible. The syntax of this language is to be discovered by careful analysis of linguistic usage in science. And what unity there is in science is to be found in the unity of its logic and syntax.

2. Linguistic analysis has revealed the all-important distinction between the *formal* and the *empirical* aspects of science. Formal science consists of the analytic statements established by logic and mathematics; empirical science consists of the synthetic statements established in the different fields of factual knowledge.

3. The statements of logic and mathematics derive their validity from conventions, and, from the point of view of empiricism, they are materially empty and constitute a closed system of tautologies. Logic deals with language only—not with the objects of language. Likewise, mathematics deals with symbols—not with the objects which the symbols represent.

4. Empirical propositions have meaning when there exists a concrete procedure (a set of operations) for determining their truth or falsity. Empirical significance attaches only to testable or confirmable sentences.

5. What we have called the 'truth' of an empirical proposition is something which can never be absolute. Repeated tests of an object-sentence can add to its probability but never clinch its certainty. Induction, as Hume pointed out, is not a watertight method of proving anything empirical.

6. The notion that all scientific sentences are translatable into a common form—the physical language—requires of psychology a behavioristic approach. Psychology so conceived is called *behavioristics.*

These alleged achievements of the philosophers have been attained in the same spirit professed by the operationists: an empirical study of the actual doings of science-makers. Little wonder, then, that the two groups, although differing in emphasis, have arrived at substantially the same generalizations. Furthermore, these studies investigating the science-makers are the beginnings of a Science of Science. Like all other sciences, this one began before it was founded. Its founding and christening are of very recent date. They coincide with the harvesting of its first fruits.

THE SCIENCE OF SCIENCE

These first fruits of the Science of Science, it would appear, are the positive advances of operationism, of Logical Positivism, and of all who have looked seriously into the rules under which science is created. Except for these fruits, of which many are still green and some may even turn out to be wormy, the Science of Science comprises little more than an optimistic program. The fullest account of this program is supplied by C. W. Morris in his excellent essay on the "Foundations of the theory of signs" (42).

Morris is a philosopher at Chicago, and many will want to ask: What good is a science in the hands of philosophers? The obvious retort is that all our major sciences passed their childhood in the mansion of philosophy and only after they had grown tough and empirical were they bold enough to desert the tender-minded parent. It may be that once again a band of curious men have turned up in some unsuspected corner a new science with which they will charm away a few hardy scholars and leave the parental mansion tenanted by the tender-minded.

Let us turn now to an outline of the scientific study of science. Morris calls it " Metascience " or " Scientific Empiricism." Morris is enthusiastically full of new terms; in fact, a difficulty with his account is that he is overly generous in his willingness to enrich our vocabulary. Much of his coinage, however, is choice and merits more extensive circulation. Morris defends the thesis that *it is possible to include without remainder the study of science under the study of the language of science, because the study of that language involves not merely the study of its formal structure but its relation to the objects it designates and to the persons who use it.* Language is a system of signs or symbols and the general science of signs is to be called *Semiotic*. Semiotic has a double relation to the other sciences: It is both a science among the sciences and an instrument of the sciences. It is not a ' superscience ' but rather a common science among the others. Every scientist at some stage of his work must embody his results in linguistic signs, and consequently he must be as careful with his linguistic tools as he is in designing his apparatus or in making his observations. In his enterprise, the scientist unites empiricism with methodological rationalism, and Semiotic studies how this marriage is consecrated.

The study divides itself into three dimensions or levels, which we shall discuss in turn:

1. Syntactics is the study of the relation of signs to signs.
2. Semantics is the study of the relation of signs to objects.
3. Pragmatics is the study of the relation of signs to scientists.

Syntactics refers to the formal disciplines commonly called logic, mathematics, and syntax, where the relation of signs to one another is *abstracted* from their relation to objects and to users or interpreters. At present this is the best developed branch of Semiotic, but in the field of the logical syntax of language there is still great labor to be done. The investigation of language from the syntactical point of view is at once both complex and fruitful. It has been possible accurately to characterize primitive, analytic, contradictory, and synthetic sentences, and to show that many sentences which are apparently object-sentences (and so concern things which are not signs) turn out under analysis to be pseudo object-sentences which must be interpreted as syntactical statements about language. An astonishing number of the scientist's sentences are syntactical in this sense (see Appendix I). They are propositions without material content.

Ayer (**1**, p. 63) gives us a "striking instance" of the way in which propositions which are really linguistic are often expressed in such a way that they appear to be factual. At first glance, the proposition, "A material thing cannot be in two places at once," looks quite empirical, but critical inspection shows that "it simply records the fact that, as a result of certain verbal conventions, the proposition that two sense-contents occur in the same visual or tactual sense-field is incompatible with the proposition that they belong to the same material thing." The proposition, then, is a definition—it records our decision as to how we shall use the term "material thing." As this example suggests, the scientist frequently couches in the material idiom the propositions which he really intends as definitions, and thereby he tends unwittingly to generate pseudo problems out of his use—or misuse—of signs.

Of course, science is not the only activity in which we use signs. The artist, the musician, and the traffic cop are notable sign-users. What their various signs express or designate concerns semantics; what the effect of these signs is on society and the individual concerns pragmatics; but we can also inquire under what rules the signs are made, combined, and transformed, and that is syntactics.

Semantics refers to the rules determining under what condition a sign is applicable to an object or situation. Thus, the operational rule [7] laid down by Bridgman for determining the meaning of a term is, I take it, essentially a *semantical rule*. And the so-called "applicational definitions" used by the Logical Positivists to state when a term shall apply to an object come under this heading (cf. Blumberg and Feigl, **8**). Within the study of these rules belong all the problems relating to the correlation between the signs which comprise a scientific treatise and the discriminable aspects of the physical world to which the signs are meant to apply. The simplest semantical rule is that governing an *indexical* sign. Such a sign designates what is pointed at at any instant. The denotation of the sign is based upon the operation of pointing, which in turn, of course, involves an act of discrimination. We have already noted that discrimination is the simplest and most basic operation performable.

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[7] In discussing operationism I have used the words *term* and *proposition*, *applicability* and *truth* (Stevens, **57**, **58**). In keeping with the spirit of Semiotic I ought perhaps to say that *terms* have *applicability* under semantical rules when the criteria governing their use are operational criteria. Then, sentences formed by combining these *semantically* significant terms into propositions are *empirically* significant (have truth-value) when their assertions are confirmable by means of operations. In other words, there is a justifiable distinction between the operational meaning of words and symbols (semantical significance) and the operational meaning of empirical propositions. I am not certain, however, that Morris would distinguish between empirical and semantical propositions in the same way.

Many of the problems of semantics belong to psychology. Morris sees in the experimental approach made possible by behavioristics great promise for determining the actual conditions under which certain signs are employed. Unfortunately, rules for the use of sign-vehicles are not ordinarily formulated by the users of a language; they exist, rather, as habits of behavior, and semantics wants to know what these habits are and how they come to be established. Many pertinent experimental studies have already been made by psychologists seeking the conditions of concept formation and judgments of similarity, but more are in order. Tolman's discovery of sign-gestalts functioning in the life of the rat discloses semantics among the rodents, and Lashley's effort to discover what range of patterns are considered equivalent by the rat when he uses them as signs for food directs attention to the problem of functional substitutivity (to use Professor Boring's term) among symbolic forms.

The game of chess is frequently suggested (cf. Carnap, **18**, and Reichenbach, **51**) as an example of a system of conventional formal rules applicable to concrete objects and situations. Perhaps at this point we can better illuminate Semiotic by examining this ancient pastime. First let us consider a set of signs. We shall use 3 groups of symbols: (1) the letters *a, b, c, d, e, f, g,* and *h*; (2) the numbers 1, 2, 3, 4, 5, 6, 7, and 8; and (3) certain other signs such as Kt, B, Q, K, etc. Next we shall set up conventional rules for manipulating these symbols by allowing only combinations in which 1 sign from each of the 3 groups appears, such as, for example, Kt *c* 4. This combination shall be transformable into other combinations, depending upon the first symbol, Kt. Thus:

$$\text{Kt}\ c\ 4 \longrightarrow \text{Kt}\ e\ 5.$$

But we shall not be allowed to write:

$$\text{Kt}\ c\ 4 \longrightarrow \text{Kt}\ d\ 5.$$

Now, when we have stated all the rules governing these signs, what do we have? Quite plainly, what we have is a formal system—a set of signs governed by syntactical rules. We are engaged in the pursuit of syntactics.

Anyone who is a chess player will have guessed by now that these syntactical rules were *abstracted* from the game of chess. The point is that we can abstract them in this way and study them with no reference to anything beyond themselves. On the other hand, we can use them as a 'model' to describe chess. In order to use them in this way we proceed to set up *semantical rules.* We say: Let the letters stand for the rows and the numbers for the columns of a chess board; let Kt stand for a particular small object (called a knight) which sits on a square of the board; then define Kt *c* 4 as equivalent to the statement that there is a knight on the square of coördinates *c* and 4; and define Kt *c* 4 ⟶ Kt *e* 5 as equivalent to the statement that the knight is moved from *c* 4 to *e* 5. These semantical rules are statements about the use of language—they merely record our decisions as to how we shall use certain signs—and as semantical rules they are not empirical propositions. (This distinction between semantical and empirical statements was not made sufficiently explicit in operationism, but it needs to be stressed.)

We create an empirical statement as soon as we say that Kt *c* 4 is true, *i.e.* that there is, in fact, a knight on *c* 4, because this statement can be operationally verified. We can look to see whether our knight is there on *c* 4, or elsewhere. If the knight is on *c* 4 the statement is confirmed as true and if the knight is not on *c* 4 the statement is unconfirmed and is false. On the other hand, the statement 'Kt *c a*' can never be considered an empirical proposition, because this combination of signs violates the rules of syntax and is meaningless—it cannot be tested operationally.[8]

From our game of chess we can abstract still another dimension or aspect. We can ask: What is the relation of these rules to chess players? Is the game hard or easy? What is its place in society, etc.? Here we are broaching pragmatical questions.

Pragmatics, as a part of Semiotic, studies the relation of signs to scientists. Here belong the problems as to how the scientist, as a behaving organism, reacts to signs; how science, as a social institution, interacts with other social institutions; and how scientific activity relates to other activities. This, indeed, is the aspect of Semiotic most challenging to the psychologist. It is the problem of the interpretation of signs. What is their effect on the man who sees or hears them? How do they determine behavior? How are they used and abused in shaping human destiny? A nebulous problem, one might complain, and overwhelmingly complex. "Yes, but none the less real and pressing," must be the answer.

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[8] Note the similarity between the statement 'Kt *c a*' and Ayer's example discussed above. To say that a knight cannot be on *c* and *a* at the same time is very like saying that an object cannot be in two places at once. Both statements follow directly from the rules of our syntax and are therefore nonempirical sentences.

The term "pragmatics" obviously suggests the philosophy known as pragmatism. The word was deliberately chosen to be thus suggestive. (In Semiotic we should say that the *pragmatical* aspect of the word is one of suggestiveness.) Pragmatism, more effectively than ever before, directed attention to the relation of signs to their users and assessed this relation as an aid in understanding intellectual activities. Pragmatics, as part of Semiotic, pays tribute to the achievements of Peirce, James, Dewey, and Mead, but it must not be thought identical with pragmatism as a philosophy.

Both pragmatism and pragmatics agree that the interpreter of a sign-vehicle is an organism whose 'taking-account' of the sign consists in a *habit to respond* to the vehicle as it would to the thing designated by the sign. We thus find the problem of pragmatics cast in such a form that it can be handled by behavioristics—we deliberately avoid talking about the subjective effects of signs unless these effects are disclosed by public operations. Not only do we react to the signs appearing in sober scientific propositions, but our habits of response carry over to situations where signs obey neither semantical nor syntactical rules. We are often delighted by senseless jingles and moved to strong emotions by what analysis shows to be gibberish. In propaganda, where syntax is usually not violated, but where semantical relations are sometimes distorted, the pragmatical effects (the induction of some form of behavior) may be profoundly disturbing. Clearly, psychology has a stake in the solution of all these problems arising in pragmatics.

One more facet of this many-sided problem deserves our interest. What Morris calls *descriptive pragmatics* occurs when a sign used by a person is employed as a means of gaining information about the person. The psychoanalyst studies dreams for the light they throw upon the dreamer, not to discover whether there are actually any situations which the dreams denote. Likewise, we may study the statements of newspapers and politicians, not as empirical propositions, but for their ability to disclose the faction whose interest is being served by this form of propaganda. And in much the same spirit, the psychiatrist inspects the signs used by his patient in order to diagnose an abnormality. The pragmatical aberrations found among the psychoses are extremely illuminating, for occasionally a patient lets his system of signs displace completely the objects they once stood for; the troublesome world of reality is pushed aside and the frustrated fellow gets his satisfaction in the domain of signs, oblivious to the restrictions of syntactical and semantical rules. The field of psychopathology thus holds great promise as a place to apply Semiotic and discover some of its laws.[9]

There can be no doubt that in the realm of human behavior the concept of sign holds a key place. And if, as the pragmatists contend, mental phenomena are to be equated to sign-responses, psychology bears an intimate relation to the science of signs. The theory of signs—being the coördinated disciplines of syntactics, semantics, and pragmatics—is the core of a unified science. " Indeed," exclaims Morris (42), " it does not seem fantastic to believe that the concept of sign may prove as fundamental to the sciences of man as the concept of atom has been for the physical sciences or the concept of cell for the biological sciences."

EPILOGUE

That then, in all too brief review, is the manner in which the Science of Science has been staked out. Whoever would probe the making of science can learn all the answers by inspecting thoroughly the language of science. The investigator must remember, however, that *this language is an intersubjective (public) set of sign-vehicles whose usage is determined by syntactical, semantical, and pragmatical rules.* By making the Science of Science coextensive with the study of the language of science we have set spacious bounds to this field of inquiry—there is ample room for a variety of talents, and to bring all the diverse areas under cultivation will require coöperation among the specialties.

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[9] Count Alfred Korzybski has written a bulky work called *Science and sanity* (Lancaster: Science Press, 1933), in which he contends that in the miseducation of our youth we teach them semantical rules based upon static Aristotelian classifications which they must then use in dealing with a fluid dynamic universe. Such semantical habits are enough out of tune with reality to drive many people crazy. Korzybski would cure the resulting insanity by renovating the patient's semantics. Whatever our opinion about this etiology and cure, it is plain that much of Korzybski's concern is with what Morris would call pragmatics—the effect of signs upon the users of signs.

Three features of this lusty embryonic science stand out with particular prominence.

First, the rational and the empirical elements in science are disentangled and then reassembled according to a straightforward, workable plan. The formal, rational, analytic, *a priori,* deductive side of creative thinking, which has always been so dear in the hearts of James's " tender-minded," neither rules nor is ruled by the empirical, synthetic, *a posteriori,* inductive wing. Neither side can be called a usurper when both are understood, for they are not even in competition. Their union is achieved, not after the manner of Kant, who held out for a bastard hybrid which he called the " a priori synthetic judgment," but in conformity with the relation of sign to object.

Secondly, it is proposed that in our study of the science-maker we begin with the *products* of his activity—his finished propositions—rather than with his ' experiences ' or any other phase of his earlier behavior. This is a sensible place to begin. If we were to study the manufacture of any product, such as automobiles, we should probably find it useful first to ascertain what an automobile is and then to discover the conditions under which it comes into being. Science manufactures sentences, and we, as curious mortals, ask: What is a sentence and how is it made? The *complete* answer to this question is the Science of Science.

Thirdly, does it not appear that the Science of Science must go directly to psychology for an answer to many of its problems? Is it not also plain that a behavioristic psychology is the only one that can be of much help in this enterprise? A sign has semantical significance when an organism will react to it as it would to the object which the sign supplants. The psychologist works out the laws under which different stimuli evoke equivalent reactions. Signs, as stimuli, can be combined and utilized extensively in the control and direction of behavior, both individual and social. The entire activity of the scientist as a sign-using organism constitutes, therefore, a type of behavior for which behavioristics seeks the laws. If there is a sense in which psychology is the propaedeutic science (cf. Stevens, **58**), it is undoubtedly in its ability to study the behavior, *qua* behavior, of the science-makers.

Perhaps we are too close to this young Science of Science either to judge its value or see clearly how it came to be. We shall forego the value-judgment, since it would merely disclose the author's particular prejudice (already clear, no doubt), but an observation about the movement's immediate ancestry is not entirely out of order. It now appears, in retrospect, that the Science of Science emerged as the reasonable outcome of revolutions in the three major fields: physics, psychology, and philosophy. These revolutions occurred almost independently, but a general community of spirit among them led directly to extensive cross-fertilization. Operationism as a revolution against absolute and undefinable concepts in physics, behaviorism as a revolution against dualistic mentalism in psychology, and Logical Positivism as a revolution against rational metaphysics in philosophy were the three forces whose convergence into a common effort is effected by the Science of Science.

Finally, the purpose of this review has been to call the attention of those of us who are psychologists to the critical principles involved in scientific method as evolved in recent scientific and philosophic movements. We have had little to say concretely about psychology or its facts, and undoubtedly many will be impatient with so much nonexperimental discourse. "Who cares about philosophy?" they will say. "What matters is the product of the laboratory." While such robust empiricism is admirable, we must ask the indulgence of these tough minds. We must ask them to bear with us while we inspect our logical tools as carefully as we do our other apparatus. And we must ask them to weigh the implications for psychology of this statement by Quine, the logician (**48**):

> "The less a science has advanced the more its terminology tends to rest upon an uncritical assumption of mutual understanding. With increase of rigor this basis is replaced piecemeal by the introduction of definitions. The interrelationships recruited for these definitions gain the status of analytic principles; what was once regarded as a theory about the world becomes reconstrued as a convention of language. Thus it is that some flow from the theoretical to the conventional is an adjunct of progress in the logical foundations of any science."

APPENDIX I

A Case Study in Language

To a greater extent than he probably realizes the scientist indulges in nonempirical discourse. It is necessary that he should, for he must help us to understand him by telling us how he intends to use his words and symbols. Most often, however, he so informs us by a method that is more implicit than explicit. He does not tell us which of his sentences are stating syntactical and semantical rules, but he hides these rules in sentences which have the outward appearance of material or empirical propositions. Not always does this practice make trouble for us, although sometimes the material mode of expression leads to arguments about pseudo problems which can properly be settled only by our agreeing to a uniform language-convention. At any rate, it is an interesting and instructive exercise occasionally to inspect our scientific books with a view toward segregating the syntactical from the empirical assertions. As an essay at such an exercise, let us examine a passage from J. F. Brown's important and scholarly work, *Psychology and the social order* (New York: McGraw-Hill, 1936). I shall present the passage (quoted from pp. 238–239 by permission of the author and publisher), together with a tentative analysis of the nature of each sentence. This analysis may or may not be the correct one—it is always dangerous to take a passage out of context or to neglect earlier statements by the author—but I give it more to illustrate a method than to prove anything about Professor Brown's propositions.

Passage	*Kind of Sentence*
"We must first combat the usage of the term personality as a quantitative concept.	*Syntactical:* states that the word personality is not a number-word, or cannot be modified by number-words.
We speak popularly of this person as having a 'great deal of personality,' of that person as having 'but little.'	*Empirical:* asserts something about our speech-behavior which is capable of operational confirmation or refutation.
It is true that the individual traits (aspects) of personality vary, but	*Syntactical:* aspect-words are terms which can be modified by number-words.
everyone has quite the same amount of personality.	*Syntactical:* personality is a unit-word.
Personality hence becomes a qualitative rather than a quantitative aspect	*Syntactical:* personality is an expression of kind rather than number.
of nature and may be defined *as the pattern or arrangement of the individual traits.*	*Syntactical:* personality is a class-word and includes traits.
We shall see later that	*Empirical:* a statement about the book itself—confirmable.
personalities vary in the degree of structuralization, but	*Syntactical:* personality is a class-word including structures. (This type of sentence might, of course, be *empirical,* provided the terms personality and structuralization have been *independently* defined elsewhere.)
this variance in degree of structuralization is more concerned with pattern than with amount.	*Syntactical:* structuralization is more a relation-word than a number-word.
Secondly, we as psychologists are not interested in whether the personality is 'good' or 'bad.'	*Empirical:* states something about us: we are indifferent about the syntactical relation between the word "personality" and value-words.

Scientifically we should be able to account for both sorts on the basis of our principles.	*Syntactical:* the relation between personality-words and value-words is implicit in our syntactical rules.
Thirdly, we must combat the idea that the individual personality is a constant aspect of the individual.	*Semantical:* the same personality-word does not always go with the same individual.
One's personality changes as the pattern of traits changes.	*Syntactical:* personality is a relation-word.
John Smith . . . has not, from the cradle to the grave, the same definite and precise personality in the scientific sense of the word.	*Semantical:* different personality-words apply to John Smith at different times.
When a normal individual goes insane, his personality changes.	*Semantical:* when a person acquires a psychosis we apply a new personality-word. (This type of statement would not be semantical provided we had *previously* given *independent* definitions to insanity and to personality, for then we should have an empirical proposition of the 'if-then' form.)
When he changes his political affiliations, his personality likewise changes.	*Semantical:* when a person changes politics we apply a new personality-word.
Marriage or divorce or parenthood in its turn brings about personality changes."	*Semantical:* these are additional rules for applying different kinds of personality-words.

We see, therefore, that most of this passage deals with the relation of the word "personality" to other words (syntactical rules) or to situations (semantical rules). These rules are essentially conventions and can be neither proved nor disproved by appeal to experiment or observation.

Most of these propositions are presented in what is sometimes called the material mode, and the brief paraphrase in the right-hand column is more nearly in what may be called the formal mode. The paraphrase is not adequate in every case to carry the full significance of the original proposition, but presumably it could be made adequate if necessary. The advantage of the formal mode is that it more readily reveals contradictions or deficiencies among our syntactical and semantical rules.

APPENDIX II

A Note on the Hypothetico-deductive Method

Those who contend that science should be pursued in the manner of Galileo rather than of Aristotle advance as the distinctive feature of Galileo's procedure the hypothetico-deductive method. Under this method we set up hypotheses from which we deduce consequences, and these we test by experiment. There can be no doubt that this is the method of science at its best. How, then, does this method concord with the picture of science presented by operationism?

The scientist fits a formal model (language, mathematics, logic) to his observations. He does more, however, than merely set down semantical rules so that his model will provide a passive description of what he has done in his laboratory. He tries to select or create a model which has a set of rules governing the manipulation of the signs or elements of the model, and he does this in order that he shall be able to exploit the model and arrive at new, testable propositions.

In terms of hypotheses and deductions the procedure is this: We set up an array of symbols in the form of an equation or proposition (hypothesis). By means of semantical rules we relate these symbols to observable events. Then, by means of the syntactical rules governing our system of symbols, we proceed to deduce; that is to say, we apply our rules of transformation and convert our original equation or proposition into others consistent with it. We then relate the terms of these new propositions to observable events by means of semantical rules, and having done this, we are in a position to confirm by experimental observation the results of our deductions.

The point to be noted is that the process of deduction is carried out at the formal level. A deduction consists in a transformation of a sentence in accordance with syntactical rules. When our hypothesis is an equation and our syntactical rules are the laws of algebra, there can be no doubt that the procedure is as outlined here. When our hypothesis is a sentence in English, where the rules of syntax are less explicitly stated, we often make deductions without being fully aware of the 'logic' we are using. Nevertheless, if we are consistent, we are using a logic of some sort, and it is the carrying out of the rules of this logic which is the process we call deduction.

Clearly, then, the hypothetico-deductive method depends for its effectiveness not only on our ability to hit upon good hypotheses (a matter sometimes of hunch or inspiration or chance), but also upon the existence of a formal calculus or language whose explicit rules of transformation make effective deductions possible. The scientist should have as large a concern for the syntax of his language and the rules of his mathematics as he has for the equipment of his laboratory.

(It will be interesting, in this connection, to see what effect on the hypothetico-deductive method in biology will eventually result from Woodger's effort (**66**) to employ a more rigorous logical syntax in the description of biological phenomena, and from Lewin's effort (**30a**) to create a new formal system—"hodological space" or path geometry—for the description of the "life space" of behaving individuals.)

BIBLIOGRAPHY

In the hope that an annotated bibliography will prove more useful than a bare list of references, I have tried to indicate for some of these works the particular features which recommend them for consideration in connection with the present review. Not all of these features have been dealt with in the text and to that extent some of the following comments are supplementary to the review.

1. Ayer, A. J. Language, truth and logic. London: Gollancz, 1936.

   This is a medium-sized book of more than medium quality by a young English disciple of the Vienna Circle. Ayer went to Vienna for a couple of years when he was twenty-two, and he wrote this book before he was twenty-five. In the course of 248 pages, he gives a simple and readable account of the rightful rôle of philosophy as the Logical Positivist sees it. He disposes in rather cavalier fashion of metaphysics and of the assumed right of rational, deductive procedures to legislate for science. He distinguishes sharply between the *formal* methods of mathematics and logic, where only tautologous statements are possible, and the *empirical* methods of science, where all inductive propositions are "hypotheses" having one or another degree of probability.

2. Bell, E. T. Men of mathematics. New York: Simon & Schuster, 1937.

   A delightful revelation of the humanity behind mathematics. It is interesting to trace in this work the progress of mathematics, not only in its concrete achievements, but also in its growing appreciation of itself as an essentially tautologous system resting on a set of fundamental conventions or postulates. The ancients tried to check their mathematics against nature; the moderns evolved so much mathematics (algebraic ideals, abstract spaces, much of number-theory, generalized geometries, etc.) which has no known application to the observable world that they had to recognize its basis in convention rather than its ability to picture empirical nature.

3. Benjamin, A. C. An introduction to the philosophy of science. New York: Macmillan, 1937.

4. Benjamin, A. C. The operational theory of meaning. *Phil. Rev., N. Y.*, 1937, **46**, 644–649.

   Here are listed certain advantages to philosophy of operational criticism.

   "The operational theory is essentially methodological rather than metaphysical." The definition of *operation* is extended by this author to include what he calls both extensional and intensional operations.

5. Bentley, A. F. The positive and the logical. *Phil. Sci.*, 1936, **3**, 472–485.

   Although agreeing with the announced aims of Logical Positivism, Bentley finds some of the works of its prophets to be seriously unworkmanlike. He finds instances in which they are neither logical (consistent) nor positive (agreeing with the methods of recognized science).

6. Bentley, A. F. Physicists and fairies. *Phil. Sci.*, 1938, **5**, 132–165.

   Bentley wonders why physicists are so foolhardy as to risk excursions into "epistemologizing." "The kind of psychology that the physicist is most apt to get his fingers on deals mostly with fairies, sprites and spooks." This paper is a review of the experiences of operationism among physicists. The author begins with Bridgman, who at least began with a sound idea. Margenau criticized Bridgman, and Bentley criticizes Margenau. Then Lindsay criticized Bridgman, and Bentley criticizes Lindsay. And then Bridgman wrote his book on *The nature of physical theory,* and Bentley criticizes Bridgman. There are, nevertheless, many interesting points made in the course of this round of criticism.

7. BILLS, A. G. Changing views of psychology as science. *Psychol. Rev.*, 1938, **45**, 377–394.

Here we are treated to an interesting address pointing out both the achievements and dangers of psychology's attitude toward method and principles.

8. BLUMBERG, A. E., & FEIGL, H. Logical positivism. *J. Phil.*, 1931, **28**, 281–296.

This is an article introducing Logical Positivism to American scholars. It is a good statement of the *early* position of the Vienna Circle.

9. BOAS, G., & BLUMBERG, A. E. Some remarks in defense of the operational theory of meaning. *J. Phil.*, 1931, **28**, 544–550.

These authors coin the word "operationalism" which they attribute to Bridgman. Bridgman, incidentally, dislikes calling the operational method an 'ism,' but the word "operationism" is probably too useful to be suppressed.

10. BORING, E. G. Temporal perception and operationism. *Amer. J. Psychol.*, 1936, **48**, 519–522.

Here we see how a traditional problem in psychology turns out to be specious in the light of operational criticism.

11. BRIDGMAN, P. W. The logic of modern physics. New York: Macmillan, 1928.

In this book appears the original statement of the operational view and an elaborate application of it to specific concepts in physics. It is this important and able book which establishes Bridgman as the father of the operational movement in physics.

12. BRIDGMAN, P. W. A physicist's second reaction to Mengenlehre. *Scripta math.*, 1934, **2**, 3–29.

In this article Bridgman attempts to apply operational criticism to class-theory: "Mathematics has meaning only in so far as it is amenable to the same sort of an operational control that has been found necessary in physics." Although this paper contains many important contributions to operationism, we must criticize its occasional failure to make an adequate differentiation between *formal* and *empirical* disciplines. Bridgman would seem to support the very questionable view that deductive propositions are not fundamentally different from inductive propositions.

13. BRIDGMAN, P. W. The nature of physical theory. Princeton: Princeton Univ. Press, 1936.

In these lectures operationism (a word Bridgman never uses) is expounded and amplified. Here we encounter excursions into psychology and epistemology and the outcome is perhaps not so happy as that of *The logic of modern physics.*

14. BRIDGMAN, P. W. Operational analysis. *Phil. Sci.*, 1938, **5**, 114–131.

This article is aimed principally at answering some of the criticisms which the author has previously encountered.

15. BRUNSWIK, E. Psychology as a science of objective relations. *Phil. Sci.*, 1937, **4**, 227–260.

Brunswik reviews the problem of how the organism deals with its environmental world, how it exhibits the "constancy phenomenon," and how it "attains its objects." He shows how many of the older problems of psychology turn out to be pseudo problems when we make the proper operational shift in their definition. This important paper is handicapped, however, by an involved style of English.

16. BURES, C. E. The concept of probability. *Phil. Sci.,* 1938, **5**, 1–20.

This is an excellent critical survey of recent contributions to the concept of probability. I call attention to it here because of the rôle that probability seems destined to play in our notions of the truth-value of empirical propositions (cf. Reichenbach, **51**). After reviewing the other major theories Bures summarizes the case for the frequency interpretation of probability and points out its basic importance.

17. CAMPBELL, N. R. Physics: the elements. Cambridge: Univ. Press, 1920.

This has long been the standard work on the logic and the operational foundation of measurement in science. Part II, dealing with measurement, numbers, dimensions, errors, etc., is still of first importance. Part I, however, dealing with the nature of laws, theories, measuring, and truth, contains some statements which are candidates for revision. Nevertheless, it is impressive to note by how much this large and careful book anticipated the views and arguments of those interested in operationism, Logical Positivism, and the Science of Science generally.

18. CARNAP, R. On the character of philosophic problems. *Phil. Sci.,* 1934, **1**, 5–19.

This, the first paper appearing in the *Philosophy of Science,* gives a brief statement of the thesis that philosophy is the logic of science and proceeds to demonstrate the differences between the two modes of speech "*inhaltliche und formale.*"

19. CARNAP, R. Philosophy and logical syntax. London: Kegan Paul, 1935.

This tiny book contains the content of some lectures delivered at London. Carnap here explains the method of philosophizing used by the Vienna Circle: the "syntactical analysis of scientific language." He also discloses some of the implications of this method, such as Physicalism and the unmasking of metaphysics.

20. CARNAP, R. Les concepts psychologiques et les concepts physiques sont-ils foncièrement différents? *Rev. Synthèse,* 1935, **10**, 43–53.

Carnap examines the proposition, "Monsieur A est en colère," in order to show that it can be translated into a "physical language." He presents an interesting analogue in the form of a proposition stating: "Dans ce conducteur passe maintenant un courant d'intensité 5." There is a class of at least a half-dozen ways of verifying this latter proposition, and sentences based on any one of these ways can be translated into sentences based on any other. Similarly there is a whole class of methods for verifying the proposition about anger, and the corresponding propositions are translatable in a manner that is equipollent (*i.e.* reciprocally deducible).

21. CARNAP, R. Testability and meaning. *Phil. Sci.,* 1936, **3**, 419–471; 1937, **4**, 1–40.

Here Carnap treats the problems of confirmation, testing, and meaning, all of which rest upon some degree of testability or confirmability, or both. We are free to choose our criteria in these matters. The notion of verifiability, if by verification we mean a *complete* and definitive establishment of truth, must be replaced by the notion of *degrees* of confirmability. A sentence is testable if we know a *method* for testing it; it is confirmable if we know under what conditions the sentence *would be* confirmed, *i.e.* how our experiment would have to turn out. A sentence may therefore be confirmable without being testable.

This long article (published in two parts in separate issues of the journal) goes on to a logical analysis and then to an empirical analysis of confirmation and testing. The last section deals with the construction of a language system. Throughout this paper we find frequent modifications of Carnap's earlier views.

22. CARNAP, R. Logical syntax of language. London: Kegan Paul, 1937.

This book is a translation of what is probably Carnap's most important work to date. Considering language as a calculus, the author sets out to systematize its rules or syntax and thereby provide the tools for working out the logic of science. The difference between the *material* and the *formal* mode of speaking is set forth with interesting examples of how it happens that many sentences which appear to be material (empirical) turn out under analysis to be syntactical (formal). These 333 densely packed pages deserve careful study.

23. DINGLE, H. Through science to philosophy. Oxford: Clarendon Press, 1937.

Dingle gives us a lively account of recent developments in the Philosophy of Science, but his criticisms of operationism appear definitely tender-minded.

24. EINSTEIN, A. On the method of theoretical physics. *Phil. Sci.*, 1934, **1**, 163–169.

Einstein tells us that if we would know the method used by theoretical physicists we must not listen to what they say they do, but examine their achievements. He further shows how these physicists go about fitting a formal model to their observations.

25. FEIGL, H. The logical character of the principle of induction. *Phil. Sci.*, 1934, **1**, 20–29.

Feigl holds that the principle of induction is neither analytic nor synthetic: "It is, rather, the principle of a procedure, a regulative maxim, an operational rule." It is something like a habit. This is a pragmatic view.

26. FEIGL, H. Logical analysis of the psycho-physical problem. *Phil. Sci.*, 1934, **1**, 420–445.

In this article, the distinction is made between a language of data and a language of constructs. (For a criticism of this dichotomy see Stevens, **57**.)

27. HEMPEL, C. G. Analyse logique de la psychologie. *Rev. Synthèse*, 1935, **10**, 27–42.

This paper contains one of the best statements available of the implications for psychology of the thesis of Physicalism. Logical analysis of the language of science demonstrates, according to Hempel, that all meaningful psychological propositions can be translated into propositions containing no "psychological concepts." "La psychologie fait partie intégrante de la physique." Any distinction between physics and psychology must be made solely for practical reasons.

28. Hume, D. Enquiries concerning the human understanding and concerning the principles of morals. (2nd ed.) Oxford: Clarendon Press, 1902.

29. James, W. Pragmatism. New York: Longmans, Green, 1914.

James's popular statement of pragmatism done in the inimitable Jamesian manner.

30. Kantor, J. R. The operational principle in the physical and psychological sciences. *Psychol. Rec.*, 1938, 2, 3–32.

Kantor here develops his brand of operationism. He is unhappy about the latent subjectivism in previous versions of it, especially that of Stevens. He wants complete 'objectivity' but tries to get it by invoking what would appear to be essentially a realism. Apart from this possible defect, this paper is rich in valuable applications of the operational attitude.

30a. Lewin, K. The conceptual representation and the measurement of psychological forces. *Contr. psychol. Theor.*, 1938, 1, No. 4, 1–247.

Lewin is looking for a formal model capable of representing the human individual in a social environment. Since Euclidean geometry is too specialized and restricted to depict forces and movements in the social field, Lewin turned to the most generalized of geometries—topology. This geometry, however, is so general and its rules so scanty that it is somewhat barren of predictive power when applied to the social field. In order to provide a geometry intermediate between topological and Euclidean, Lewin has, in the present work, developed what he calls hodology. Hodology is the geometry of paths—a geometry in which distance and direction are defined, but not in metrical terms. As a formal model applicable to the "life space" of the individual, hodology holds great promise for psychology.

31. Lindsay, R. B. A critique of operationalism in physics. *Phil. Sci.*, 1937, 4, 456–470.

Here is where Lindsay accuses operationism of the kind of "particularism" which would "imply the abandonment of the method of theoretical physics."

32. Lindsay, R. B., & Margenau, H. Foundations of physics. New York: Wiley, 1936.

An important advanced textbook on physics in which the authors do us the great service of giving us not only formulas but a critical discussion of their meaning, justification, and limitations. Here we find the "text" which Bridgman claims should go with every mathematical formula.

33. Lundberg, G. A. Quantitative methods in social psychology. *Amer. sociol. Rev.*, 1936, 1, 38–54.

A plea is made for an operational rather than an intuitional approach to the problems of the social sciences.

34. Malisoff, W. M. The universe of operations (a review). *Phil. Sci.*, 1936, 3, 360–364.

This is an interesting critical review of Bridgman's *The nature of physical theory.*

35. Margenau, H. Causality in modern physics. *Monist,* 1931, **41**, 1–36.

Margenau, in the course of an extensive examination of the logic and meaning of causality as it appears in modern physics, makes the observation that operationism would, if carried to its consequences, land us in the midst of discrete concepts lacking logical coherence.

36. Margenau, H. Methodology of modern physics. *Phil. Sci.,* 1935, **2**, 48–72; 164–187.

A heavy, but able, inquiry into the problem of abstractness in modern physics.

37. McGeoch, J. A. Learning as an operationally defined concept. *Psychol. Bull.,* 1935, **32**, 688.

38. McGeoch, J. A. A critique of operational definition. *Psychol. Bull.,* 1937, **34**, 703–704.

McGeoch raises two pertinent questions for operationism: that of its ability to deal with qualitative discrimination and that of the operational definition of operations.

39. McGregor, D. Scientific measurement and psychology. *Psychol. Rev.,* 1935, **42**, 246–266.

A careful and definitive consideration of the problems and the logic of measurement in psychology. McGregor worked in close collaboration with Professor Boring in producing this paper. The discussion demonstrates how the meaning of measurements rests upon the operations performed in measurement.

40. Menger, K. The new logic. *Phil. Sci.,* 1937, **4**, 299–336.

All that the mathematician does "is to deduce propositions by certain methods (which must be formulated) from certain initial propositions (which must also be formulated). There are a variety of ways to choose both the methods and the initial propositions." The mathematician, as a mathematician, cannot say what is the relation of these propositions and methods to so-called reality.

41. Morris, C. W. Scientific empiricism. *Int. Encycl. unif. Sci.,* 1938, No. 1, 63–75.

This is a brief statement of the attitude of scientific empiricism, which embraces at once radical empiricism, methodological rationalism, and critical pragmatism. It is the application of this point of view to science itself that, according to Morris, generates metascience, or the Science of Science.

42. Morris, C. W. Foundations of the theory of signs. *Int. Encycl. unif. Sci.,* 1938, No. 2, 1–59.

In this article Morris defines the scope and aims of the science of Semiotic, or the science of signs. The study of the language (signs) used by science is the best approach to the Science of Science.

This paper climaxes ten years of revolution in science and philosophy.

43. Nagel, E. Some theses in the philosophy of logic. *Phil. Sci.,* 1938, **5**, 46–51.

An address in which Nagel points out the growing scientific outlook among philosophers.

44. Neurath, O. Historische Anmerkungen. *Erkenntnis,* 1930, **1**, 311–314.

The author gives us an orientation toward Logical Positivism in terms of its antecedent thinkers and its present near relations.

45. Neurath, O. Physicalism: the philosophy of the Viennese Circle. *Monist*, 1931, **41**, 618–623.

This paper gives a brief, but somewhat discursive, statement of the physicalist doctrine whose name is due to the inventiveness of Neurath.

46. Neurath, O. Unified science and its encyclopedia. *Phil. Sci.*, 1937, **4**, 265–277.

Neurath, who appears to be godfather to the new *International Encyclopedia of Unified Science,* here describes its purpose, background, and plan of development.

47. Poincaré, H. The foundations of science. New York: Science Press, 1913.

"All the scientist creates in a fact is the language in which he enunciates it."

48. Quine, W. Truth by convention. In *Philosophical Essays for Alfred North Whitehead.* New York: Longmans, Green, 1936. P. 90.

49. Rashevsky, N. Foundations of mathematical biophysics. *Phil. Sci.*, 1934, **1**, 176–196.

This, and other articles by Rashevsky, are interesting in their effort to demonstrate what can be gotten by an application of classical mathematics to biological and psychological problems. This work should be compared to that of Woodger, who applies a somewhat different logical model to similar problems. It is perhaps too early to judge the relative fruitfulness of these two approaches. For a more detailed bibliography of Rashevsky's work see *Psychometrika,* 1936, **1**, 1–26.

50. Rashevsky, N. Physico-mathematical methods in biological and social sciences. *Erkenntnis,* 1936, **6**, 357–365; and Mathematical biophysics. Chicago: Univ. Chicago Press, 1938.

Rashevsky here outlines his purpose and method quite explicitly. What he attempts to do is "to develop mathematical biology as a *rational* theoretical science, according to patterns suggested by theoretical physics."

51. Reichenbach, H. Experience and prediction. Chicago: Univ. Chicago Press, 1938.

Professor Reichenbach, formerly of Berlin, then of the University of Istanbul, and now in the United States, has brought together in this vital work the fruits of his years of work in philosophy, logic, and mathematics. He amplifies, extends, and on some points revises the philosophical outlook of those who, like the Logical Positivists, take a scientific attitude toward the traditional problems. He is interested in uniting "the empirical conception of modern science and the formalistic conception of logic," and he makes a clear restatement of the secured achievements of those interested in this same movement. He reconsiders these advances, however, in the light of the concept of *probability.* Probability has long been Reichenbach's forte, and here he applies it to the central problems of scientific induction. Empirical propositions always have a degree of probability which is less than certainty. Furthermore, probability has meaning only when interpreted as a relative frequency (contrary to Keynes' view). These two notions can be harmonized, says Reichenbach. (Read Chapter 5 to see how he achieves this harmony.) Chapter 5, incidentally, contains a résumé of his important and well-known work "*Wahrscheinlichkeitslehre*" (Leiden, 1935).

The earlier chapters deal with meaning, impressions, constructs, etc. Especially to be recommended is his use of the game of chess to illustrate the three predicates of propositions: their meaning (verifiability), their truth-value (operational test), and their predictional value (probability or "weight").

52. Rosenzweig, S. Schools of psychology: a complementary pattern. *Phil. Sci.,* 1937, **4**, 96–106.

53. Schlick, M. De la relation entre les notions psychologiques et les notions physiques. *Rev. Synthèse,* 1935, **10**, 5–26.

An examination of the relation between psychological and physical concepts in the light of the thesis of Physicalism.

54. Seashore, R. H., & Katz, B. An operational definition and classification of mental mechanisms. *Psychol. Rec.,* 1937, **1**, 3–24.

55. Somerville, J. Logical empiricism and the problem of causality in social sciences. *Erkenntnis,* 1936, **6**, 405–411.

First of all, the author points out that the social sciences are to be included in the unity of science on an equal footing with the natural sciences. He then argues that "this unity is organic, and is to be found in the criteria, method, and language of science."

56. Stevens, S. S. The operational basis of psychology. *Amer. J. Psychol.,* 1935, **47**, 323–330.

The contents of this and the two following papers have been outlined in the body of the present paper.

57. Stevens, S. S. The operational definition of psychological concepts. *Psychol. Rev.,* 1935, **42**, 517–527.

58. Stevens, S. S. Psychology: the propaedeutic science. *Phil. Sci.,* 1936, **3**, 90–103.

59. Struik, D. J. On the foundations of the theory of probabilities. *Phil. Sci.,* 1934, **1**, 50–70.

The conflicting claims of the "schools" of probability (due to different synthetic judgments) mean for Struik that "the existence of a mathematical theory of probabilities remains a kind of miracle, as esoteric to the further domain of natural science as the resurrection of the dead."

60. Tolman, E. C. Purposive behavior in animals and men. New York: Appleton-Century, 1932.

This well-known work is an interesting attempt to give explicit operational definition of terms ordinarily considered 'mental.'

61. Tolman, E. C. An operational analysis of 'demands.' *Erkenntnis,* 1936, **6**, 383–390.

A demand (desire, wish, purpose, need, motive) is a "constructed" variable in psychology and it can be operationally defined. Demands are of three kinds: primary appetite demands, primary aversion demands, and secondary demands. All three types can be demonstrated in the rat, where, as Tolman shows, operational definition is easy. He is, as a matter of fact, uneasy about the utility of verbal report:

"My motto for the present is:
Rats, not men
Gross behavior, not verbal reports."

(Fortunately, all psychologists are not so ready to sell out to the rodents.)

62. Tolman, E. C. Operational behaviorism and current trends in psychology. *Proc. Twenty-fifth Anniv. Celeb. Univ. S. Calif.*, 1936, pp. 89–103.
63. Waters, R. H., & Pennington, L. A. Operationism in psychology. *Psychol. Rev.*, 1938, **45**, 414–423.

    In this critical review, the authors object to Bridgman's principle on these grounds: (1) the principle lacks novelty; (2) it does not guarantee unanimity in psychology; (3) it leads to multiplication of the number of concepts; and (4) it unnecessarily restricts the scope of operational definitions.
64. Weinberg, J. R. An examination of logical positivism. London: Kegan Paul, Trench, Trubner, 1936.

    This is, at present, the most extensive critical review of the work of the Vienna Circle. Weinberg begins with the theories of Wittgenstein and traces the development of Logical Positivism through Physicalism. He points out the various difficulties encountered by the movement at successive stages and shows how these difficulties were met or failed to be met.
65. Wittgenstein, L. Tractatus logico-philosophicus. New York: Harcourt, Brace, 1922.
66. Woodger, J. H. The axiomatic method in biology. Cambridge: Univ. Press, 1937.

    Here is a book whose aim must command the attention of all psychologists who have followed with interest the efforts of certain workers, like Lewin and Hull, to adapt a formal scheme to the representation of psychological data. This book is an experiment in the application of a type of logistic to biology. Woodger, with the help of logicians Carnap and Tarski, tries to "provide an exact and perfectly controllable *language* by means of which biological knowledge may be ordered." He believes that if we have a perfect language we need not dispute—we need only calculate and experiment. The symbols of the biological calculus developed in this book are those of modern logistic. The impression is almost inevitable that Woodger's approach, beginning with the problem of an exact 'language' and then applying that 'language' to biology, is one that could be used in psychology. Perhaps this approach to Hull's "psychological system" would make his efforts even more fruitful. (Since first writing this note, I find that Woodger has already made a tentative effort to "formalize" Hull's system. (Woodger, J. H. The formalization of a psychological theory. *Erkenntnis*, 1938, 7, 195–198.)

SUGGESTION AND SUGGESTIBILITY[1]

A BIBLIOGRAPHY

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In spite of excellent bibliographical services available to psychologists and other professional groups such as political scientists and sociologists, a research worker in any of these groups must spend an exorbitant amount of time tracking down literature bearing upon his problem. Titles of articles often hide rather than reveal the methods used, the subjects employed, and the concepts derived. It was considered desirable, therefore, to prepare an analytical index for several different topics broadly conceived as falling within the area of social psychology. This is the first in a series under preparation. Although an attempt has been made to include all pertinent references published in English, no claim can be made for completeness. The field of hypnosis has been avoided except where it definitely concerns comparisons or contrasts with suggestibility. If the bibliographies prove useful to colleagues, and if the generosity of the Federal Government can be extended to provide workers with appropriate skill, it might be feasible to analyze articles in foreign journals. As new bibliographies are prepared, they will be offered for publication.

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AUTOMOBILE DRIVERS CAN BE IMPROVED

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In an article in this journal,[1] Harry M. Johnson and Percy W. Cobb question the educational value of drivers' clinics as sponsored independently by Slocombe, Lauer, and the writer on the basis of published evidence. Speaking for myself, I have confidence in the possibility of rehabilitating drivers who have had accidents, in spite of Johnson's unbounded pessimism that they are "born to crash," and that the only way to cure them is to drastically and permanently remove perhaps 5%, or some 2,000,000, drivers from the road.[2] Since, however, facts speak stronger than words, I shall summarize here 2 investigations bearing on the problem of the possibility of re-educating drivers.

In recent articles,[3, 4] Forbes and the author have summarized the educational results of a driver clinic administered to a number of accident drivers who were called in by the Manchester, New Hampshire, Police Department. A total of 180 names were picked from the accident file of drivers who had had 2 or more accidents during the preceding 15 months. On account of limited time at our disposal we were able to test and interview only 101 of these drivers. The 79 other drivers received the benefit of the educational work of a Safety Campaign. The 2 tables below indicate the improved driving record of the drivers attending the clinic as compared with other equally bad drivers not attending the clinic, both from the standpoint of total accidents (Table I) and drivers involved (Table II).

Another article[5] reviews the educational effects of a driver clinic (involving test and educational interview) on the accident records of 163 bus drivers of the Boston Elevated Railway. Table III summarizes the accident records of those drivers for the year following and the 3 years preceding their clinical tests. It will be noted that there was a *reduction of 28% in chargeable accidents* of drivers during the year subsequent to attending the clinic. *This improvement took place in spite of the fact that the Boston Elevated accident rate for all drivers was 5% higher during this period than the average rate for the 3 preceding years.*

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[1] H. M. Johnson & P. W. Cobb. The educational value of "drivers' clinics." *Psychol. Bull.*, 1938, **35**, 758–766.

[2] H. M. Johnson. Born to crash. *Collier's*, July 25, 1936, **98**, 28, 58, 60.

[3] T. W. Forbes. Age performance relationships among accident-repeater automobile drivers. *J. consult. Psychol.*, 1938, **2**, 143–148.

[4] H. R. DeSilva & T. W. Forbes. Improving bad drivers. *Safety Engng*, 1938, **75**, No. 6, 13.

[5] H. R. DeSilva, R. G. Claflin, & W. J. Simon. Making safer bus drivers. *Transit J.*, 1938, **82**, No. 12, 450–451, 471.

TABLE I

RE-EDUCATIONAL EFFECT OF DRIVER TEST CLINICS

Comparison of Accident Record Before and After Clinic Operation
Tested and Untested Repeaters—9 Months
Follow-up—Manchester, N. H.—January, 1937

	Number of Accidents Accruing to Groups of Repeaters on a Basis of 100 Drivers per Group	
	Untested	Tested
Before Summons to Clinic (15 months)	195	221
After (9 months)	38	18
Accidents After as Per Cent of Accidents Before (compared on a 9-month basis in each case)	32.5%	13.7%
Advantage According to Re-education by Driver Test Clinic (compared to police attention, safety campaigns, etc., alone)		57.5%

TABLE II

RE-EDUCATIONAL EFFECT OF DRIVER TEST CLINICS

Drivers Involved in Accidents, Before and After Clinic Operation
Tested and Untested Repeaters—9 Months
Follow-up—Manchester, N. H.—January, 1937

	Untested	Tested
Drivers in 2 or More Accidents Before Summons to Clinic (15 months)	79	101
Drivers in 1 or More Accidents After (9 months)	21	16
Drivers in Accidents After as Per Cent of Those Before Clinic Operation (compared on a 9-month basis in each case)	45%	27%
Reduction by Driver Test Advantage According to Clinic		40%
Per Cent of Drivers Showing 2 Accidents Before and Any Accident After	48%	20%
Advantage of Clinic Re-education		48%

TABLE III

ACCIDENTS FOR BOSTON ELEVATED RAILWAY GROUP BEFORE AND AFTER CLINICAL EXAMINATION

	June 1937 June 1938	1936	1935	1934
Number of Chargeable Accidents	99	132	154	131
Number of Exonerations	140	131	115	146
Total Accidents	239	263	269	277

Chargeable Accidents

Number of Accidents per Year for the Past 3 Years	139
Number of Accidents from June 1, 1937 to June 1, 1938	99
Decrease in Number of Accidents	40
Percentage Improvement	28%

Total Accidents

Number of Accidents per Year for the Past 3 Years	269.7
Number of Accidents from June 1, 1937 to June 1, 1938	239.0
Decrease in Number of Accidents	30.7
Percentage Improvement	11.3%

A REPLY TO LANIER'S NOTE ON 'MOTOR SPEED AND TEMPO'

BY JOHN P. FOLEY, JR.

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In a note on the writer's [1] review of the literature on "Factors conditioning motor speed and tempo," Lanier [2] has accused the writer (1) of misrepresenting the position taken by Lambeth and Lanier [3] in their paper on "Race differences in speed of reaction," and (2) of erroneously labeling such a position as "antiquated" and "mentalistic."

In the article under consideration, the Stanford-Binet scale, a rational learning test, and 6 tests of rate of response were given to 30 White and to 30 Negro 12-year-old boys. The intercorrelations among the various tests were found to be higher in the case of the Negroes. This fact, according to Lambeth and Lanier, "seems to argue for the *relatively greater operation of a 'general factor' among the conditions which DETERMINE Negro performance*" (p. 278 of original article, quoted on p. 31 of note by Lanier; italics and caps mine). They add: "This higher intercorrelation might conceivably be due to any one of several causal factors or to a complex of factors. We have already mentioned, and dismissed as far as these data go, the possibility that greater heterogeneity in the Negro sampling might operate to produce these higher correlations. It has been suggested that the presumably more differentiated environment of the White might produce a greater specialization of 'abilities' which would result in a lower average intercorrelation among test scores for that race. *Or the Negro might be basically of a simpler, more homogeneous, more 'primitive' type of organization.* We cannot conclude from our data which hypothesis, if either, is correct, and speculation is useless. The problem must be left, as indicated above, to future research" (p. 278 of original article, quoted on pp. 31–32 of note by Lanier; italics mine).

If one were to omit all consideration of possible sampling errors, of the hazards in generalizing from such a few cases, and of the questionable use of the fictitious concept of "race," one could still question the logic involved in the second hypothesis italicized above. It will be noted that the remarks of the writer to which Lanier objects relate to this hypothesis. It will also be noted that the "fact" that "the Negro might be basically of a simpler, more homogeneous, more 'primitive' type of organization" is put forth by Lambeth and Lanier as a possible *causal factor* in the results which they obtained (*i.e.* the higher intercorrelations), not merely as a statement of the results themselves. Lanier's verbal homage to operationalism is worthy indeed, although any attempt to give an operational definition of the "causal factor" of the Negro's "simpler, more homogeneous, more 'primitive' type of organization" would indeed be a feat in symbolic logic as well as in a new and hitherto undiscovered scientific physical anthropology. Such concepts have no basis in empirical fact, are scientifically meaningless, and, I am forced to repeat, smack of mentalism and—I am optimistic enough to believe—antiquity.

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[1] J. P. Foley, Jr. Factors conditioning motor speed and tempo. *Psychol. Bull.*, 1937, **34**, 351–397.

[2] L. H. Lanier. A note on Foley's review of the literature on 'motor speed and tempo.' *Psychol. Bull.*, 1939, **36**, 31–32.

[3] M. Lambeth & L. H. Lanier. Race differences in speed of reaction. *J. genet. Psychol.*, 1933, **42**, 255–297.

To Lanier's statement that "the current concept, 'mental organization,' carries no 'mentalistic' connotations in the dualistic sense" (p. 32), I can only refer, by way of example, to Thurstone's recent critique[4] of Anastasi's monograph on the influence of specific experience upon mental organization.[5] Thurstone, an acknowledged pioneer in the field of mental organization, repeatedly alludes to the statistically derived factors as "basic human abilities" (pp. 230–231) or "mental faculties" (pp. 234–235), and regards such factors as underlying psychological entities or "faculties" which the subject *uses* when he solves a test problem or responds to any stimulus situation. Thurstone thus introduces a sharp dichotomy between *ability* and *behavior*.[6] It will be noted that this is the same type of mentalistic thinking employed by Lambeth and Lanier in their hypothesis attributing the higher Negro intercorrelations to the possible operation of "a simpler, more homogeneous, more 'primitive' type of organization." The antiquated characteristic of such doctrines should be clearly apparent to anyone familiar with the elementary principles of logic, even when these doctrines are smuggled into the framework of an otherwise empirical psychology.

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[4] L. L. Thurstone. Shifty and mathematical components: a critique of Anastasi's monograph on the influence of specific experience upon mental organization. *Psychol. Bull.*, 1938, **35**, 223–236.

[5] A. Anastasi. The influence of specific experience upon mental organization. *Genet. Psychol. Monogr.*, 1936, **18**, No. 4, 245–356.

[6] Cf. A. Anastasi. Faculties *versus* factors: a reply to Professor Thurstone. *Psychol. Bull.*, 1938, **35**, 391–395.

BOOK REVIEWS

Murray, H. A. Explorations in personality. New York: Oxford Univ. Press, 1938. Pp. xiv+761.

This volume, which was written for the most part by Professor Murray, represents "A Clinical and Experimental Study of Fifty Men of College Age by the Workers at the Harvard Psychological Clinic." The first chapter introduces the problems which are considered and, in general, the viewpoint which guided the studies.

The second chapter, "Proposals for a Theory of Personality," is a very long and involved argument advanced by Professor Murray to clarify the philosophical considerations surrounding the particular viewpoint which he has adopted. He considers in detail the advantages accruing from the study of "internal" man rather than from the study of "external" man. He considers in all its aspects the problem which others have dealt with under the concept of motivation. This, in Murray's terms, is really the problem of *need*. His concept of need is carefully differentiated from the concepts of drive, traits, attitudes, motivation, etc., so that, philosophically, his approach has all the limits stated and defined. The notion of the importance of the genetic development of the internal life of the individual in relation to the adult personality is carefully formulated. The line of argument depends on evidence and hypotheses previously developed by the psychoanalysts, but broadens them and fits them into a wider and more inclusive scheme of thought. This chapter is long and covers a wide range of psychological points. The theories and concepts are sometimes clearly expressed, but at other times the final product does not seem to justify the long arguments which have been used to maintain or elaborate the point. As a general psychological orientation it is at the same time illuminating and baffling.

The third chapter, "Variables of Personality," presents the anatomical structure which Murray proposes for use in the descriptive analysis of personality. It is extremely difficult reading and probably can be justified only in terms of the outcome. If the particular system of description and definition will lead us either now or later to a more complete and better understanding of personality, then the effort has been justified; otherwise, the conceptual framework is merely a logical play on words which seems intuitively correct but has no counterpart in reality. Whether this anatomical structure turns out to be a psychological, or merely a logical, construct remains for usage to determine.

Chapter IV, "Judgments of Personality," has been written by R. Wolf and H. A. Murray. Interesting material is presented here, bearing on the way in which opinions may be synthetically obtained from putting together both experimental findings and judgments by qualified experts. The authors point out that the data are insufficient and the sources of error many, but it seems that they have approached a problem which is central and important for all psychological work of this variety.

Chapter V, "The Genetical Investigation of Personality: Childhood Events," is an excellent statement of the factors out of infancy and childhood which are of real importance in the adult personality. Again, most of this material has been brought out and elaborated by psychoanalysts. Murray's use of this material is carefully thought through, and the orientation of it to the general field of psychology is well defined. It seems to the reviewer that a great deal of valuable thought and material is presented here which might well be incorporated into general academic textbooks.

Chapter VI, "Procedures," describes some twenty-five or thirty procedures and experiments administered by the various workers at the Clinic to the individuals studied. Some of these experiments are given in detail, with their proper psychological setting and their relation to the entire problem of personality pointed out; others are sketched in a brief and unsatisfactory fashion.

Chapter VII, "The Case of Earnst," is a long and detailed case history which presents the direct application of the philosophic viewpoint and experimental procedures which have been outlined in the previous chapters.

In Chapter VIII, "Conclusions," Murray summarizes his viewpoint concerning the value of the work reported.

First, the reviewer will present the points which he considers to be positive features of this study. The project in its entirety is psychology in the strict sense of that term. It is dealing with that aspect of human existence which everyone, except certain professional psychologists, believes to constitute the field of psychology. It deals with the inner life of the individual and the ways in which this inner life is modified by, or modifies, the environment. A working terminology is developed, and a real attempt has been made to use the terminology consistently. All too frequently such terminologies have been presented in the past and then never brought into practical application.

This is a frank, open, dynamic approach which draws much of its orientation from Freudian psychoanalysis. However, it is bigger, more inclusive, and more sophisticated than anything so far presented by the Freudians. It lacks the mysticism and poetry so inherent in previous psychoanalytic presentations. Intuition is frankly acknowledged, accepted, and given its rightful place in a scientific procedure without recourse to unnecessary spirituality.

The explorations are ingenious; most of the methods are shrewd and intelligent; the application of the method of medical consultation for diagnosis and prognosis to the problems of psychological study of personality is, generally speaking, new in psychology, and the transfer from medicine to psychology has been well done. The author has attempted to predict wherever possible. He has not hesitated to use good judgment and such knowledge as he or his colleagues possessed in stating the possible outcome of factors which they have determined.

We have had endless discussions in psychology concerning the study of the personality as a whole. Mostly, these have been verbal fireworks which were not backed up by anything more than a phrase and a desire. Murray and his colleagues have actually attempted to do what others have spoken of for years. They have orientated their problem with reference to all present psychological knowledge and have not handicapped themselves by adhering blindly to any particular preconception, such as some specialized brand of psychoanalysis, trait psychology, etc. The broadness of the approach and the attempt at the same time to keep their lines of communication open are most commendable and have produced excellent results.

The negative points which have impressed the reviewer are next in order. Personally, I dislike the terminology and the way in which the terminology all too frequently obscured the meaning which the authors were trying to present. I know that psychology stands in great need of a working vocabulary which can be used in the field of personality. It seems to me that the vocabulary which the Harvard Clinic has presented is at present awkward, at times inexact, and in part meaningless. Whether future work will make this terminology a smooth-fitting garment suitable to clothe adequately the presentation of the facts of the study of personality remains to be seen.

This volume contains an overload of philosophy and theory. The extended discourse of Chapter II seems unnecessary. Pragmatically speaking, the study should demonstrate its own worth without the long verbal defense. A statement of the viewpoint and the assumptions underlying the work is all to the good, but the compulsion to relate this viewpoint to all other varieties of knowledge seems at present a wasted effort. It is exceedingly difficult and at times impossible for the reader to distinguish between experimentally determined fact and preconceived or derived theory.

The two most glaring deficiencies in the work are in the selection of experimental methods and the statistical expression of the results. Many of the experiments are ingenious. They are new and carefully thought out. However, other experiments which are used are unbelievably naïve. When one considers the vast amount of work which has been done on learning or on the psychogalvanic reflex and then views the experiments which were supposed to bear on these two measurable aspects of personality, one feels that the work which academic experimentalists have been doing for the past eighty years has been done in vain. I suppose that this is due to the fact that certain of the workers at the Harvard Clinic were students beginning graduate work who had no concept of the vast field of experimental psychology which is available in the literature. Certainly, several of the experiments reported give no reference to the body of previously determined experimental facts which should have added a great deal of meaning to the material.

The presentation of statistical evidence is variable. Certain of the workers present tabular statements of correlation, probable error, etc.; other investigators report their results in such a fashion that only the most careful examination of the entire report gives the reader any notion concerning the true validity of the results. For example, the statement may be made that a correlation of +.40, obtained in a certain experiment, is significant. Careful reading will show that the +.40 may be based on but fifteen cases, with no statement of scatter, probable error, steps, etc. Of course, the viewpoint and the experimental procedure did not demand particularly careful or elaborate statistical presentation. However, I do believe that, in general, a more adequate mathematical treatment of the results might have altered the conclusions and probably would have brought about more positive suggestions for further work.

A diagnostic council of experts was continually informed of the outcome of each experiment in order that they might prognosticate the outcome of future experiments as well as suggest new experiments. Their work constituted the keystone of the entire study. Unfortunately, the work of this committee is not adequately reported. There is no basis for judgment of how well or how poorly this council functioned. The statement is made that the council became more accurate and adequate with practice. However, one cannot learn whether, at the end of the experimental program, they had a basis of prediction which was 90% correct or 50% correct.

The case history which is given as the proof of the method leaves much to be desired. It is a nice anatomical dissection of a personality, but it certainly is weak in functional prediction, and functional prediction was the end-product desired. Furthermore, the various experimenters who report their findings do so in differing terms. Having read through the complete case, one is impressed by the " sour " or ultrasophisticated attitude of many of the experimenters. Actually, Earnst impresses the reviewer as a commendable person with outstanding personality assets, yet if many of the workers at the Clinic did have any appreciation or understanding of these assets of Earnst, they certainly carefully concealed such knowledge and failed to report it.

The dust cover of this volume contains the phrase "Let Not Him Who Seeks Cease Until He Finds, and When He Finds He Shall Be Astonished." This really characterizes the study. It has been a gallant attempt; it is overloaded with philosophy, and suffers from poor experimental work in a fair number of experiments. Certain of the workers at the Clinic present brilliant and outstanding experimental findings and formulations which save the project as a whole. This study might be compared to an attempt to reach the top of Mount Everest. Other psychologists may learn by the failures of this expedition as well as other expeditions which have gone before. Those who took part in the expedition know better than anyone else who contributed and who failed. But over and above everything else the project again demonstrates the value of trained investigators who can make use, in an intelligent and ingenious fashion, of all previously accumulated psychological knowledge from whatever source.

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Köhler, W. The place of value in a world of facts. New York: Liveright, 1938. Pp. ix+418.

Köhler opens his book with the clever literary device of discussing with an editor friend "the case against science." The discussion is precipitated by the realization that science faces a crisis of its own making. Science has lost its esteem and has generated disaffection because its devotees, having willfully neglected the needs of everyday man, have retired to their ivory tower where they carry on their own technical pursuits. Science does not deal with values, but only with facts. According to the indictment, this is no less true of the social sciences and psychology than of physics. The remaining nine chapters are devoted to the demonstration that there is a place—and a large one—for values in a world of facts.

The *mise en scène* of Köhler's trial of science is Germany, though the conditions there are typical of all places. According to the critic, science risks the result not only of its professors being replaced by other teachers who would be followed by the people, but science itself, feeding upon negativism, will perish of its own inanition.

Though this literary device excellently serves Köhler's purpose of stating a problem designed for his own type of solution, the critical reader might be spurred on to ask the following questions: Is it true that all scientists are negativists? Do all scientists exclude sounds, sights, and other concrete qualities from the world of nature, on the plea that these are nothing but human phenomena? Does science, or do only some scientists, really divorce values from facts? Are facts merely abstract quantitative determinations and therefore *ipso facto* exclusive of the qualitative characteristics of events? Is science obliged to feed and clothe the world in addition to its help in providing the means for supplying such needs? Is science, without politics, economics, law, and custom, to be the scapegoat upon which to heap the sins of men, even of those who suffer from their neglect to learn their responsibilities as wise citizens who vote properly and deal justly with one another? These questions notwithstanding, the reader must agree with Köhler that the articulation of values with other phenomena is as interesting as it is important. In consequence, he will follow Köhler's treatise with deep absorption.

In order to establish his contention that values and facts are not sharply set over against each other, but rather that values are essential features of nature, the author critically examines (Chap. II) a number of prominent current value theories. The study of these theories results in the conclusion that value means, essentially, requiredness. What requiredness is he proceeds to show (Chap. III) by demonstrating that "certain facts do not only happen or exist, but issuing as vectors in parts of contexts, extend toward others with a quality of acceptance or rejection" (p. 100). In other words, they belong together. "Thus value-situations fall under the category of gestalt" (p. 86). This result is achieved by the phenomenological method. When employing intuition, introspection, and what philosophers call immediate experience, Köhler discovers that "fact" is an ambiguous term. "Not all facts are 'indifferent facts'." "Within certain factual contexts the requiredness or wrongness of some facts is no less real than is the existence of these facts" (p. 102).

Naturally, Köhler is not content with merely establishing values in the precarious phenomenological realm. Though "phenomenology is the field in which all concepts find their final justification" (p. 102), he cannot forego reaching out toward another and more scientific realm—the physical world. And so in Chapter IV, entitled "Beyond Phenomenology," he attempts to show how it is possible to transcend this realm. The task is accomplished by memory and its underlying neural traces. Since memory involves, essentially, a reference beyond itself, Köhler believes that not only is transcendence beyond the phenomenological world established, but also at the same time transphenomenal existence is implied (p. 118).

Not that difficulties are wanting here, for the author regards the physical world as of such a character that it is not directly accessible to man, but inferentially derived from the observation of percepts (p. 142). So great is the difference between the two that the physical world exists only through a procedure of construction (*ibid.*). Chapter V, however, is devoted to showing that there is an invariable relationship, an isomorphism, between the perceptual or phenomenological and the physical worlds. As is quite to be expected, isomorphism looms large in Köhler's thinking as a necessary and sufficient factor. It is necessary, since he rejects, for example, Eddington's idea that all observation in physics is reduced to pointer readings or that pointer readings are the only perceptual materials employed by the physicist in his research and scientific construction, thus leaving ample place for phenomenal or mental components; and it is sufficient because, by emphasizing this relationship, he deems himself capable of crossing from one to the other. Because he cannot entertain the possibility that science can dispense with such a dualism, we read between his lines the analogue to Archimedes' cry: Give me gestalt and isomorphism and I will unite the absolutely incommensurable psychical and physical.

As in his other writings, so in this volume, Köhler makes use of the brain to bring together the phenomenal or mental and the physical. Following traditional dualism he relies upon the nervous system to make requiredness or value a factual as well as a phenomenal or mental affair. In Chapter VI the author elaborately expands his doctrine of psychophysical, really psychoneural or psychocortical, isomorphism. Characteristically, the fundamental point is, of course, that there is a resemblance between experience itself and the cortical correlates of experience. "The fact which mediates between the physical and the perceptual structure is now found to be *cortical* organization, which, as a rule, resembles both" (p. 218).

In Chapter VII, entitled "On Memory and on Transcendence," the author applies his psychocortical theory in the form of traces to establish requiredness and the coincidence between facts and values. He argues that not only is there transcendence in simple, successive comparison as well as in long-range memory, but there is requiredness also, as when, for example, we try to recall a name and realize that a standard beyond informs us that this name is wrong and that one is closer, etc. (p. 276). "Once more we have an amphibian context; requiredness is its most striking trait; and the vector, which in this context objects to one thing or readily accepts another, owes its character as a quite specific demand entirely to the properties of a trace" (*ibid.*).

After a rather thorough examination of the general organic field (Chap. VIII), Köhler concludes that "no procedure of science reveals any actual participation of demands and values in the determination of organic events" (pp. 327 f.). But despite the fact that we do not find requiredness as such among the data of science, a general trend of nature may yield the same results as if events occurred in order to fulfill a demand.

In the field of forces, however, Köhler finds great similarity to requiredness (Chap. IX), since forces constitute factors in dynamic contexts or situations in which they transcend from certain parts to others and point beyond present existence to future changes. Accordingly, the author is satisfied that nature comprises a dualism of facts and oughts.

In his closing chapter Köhler considers the general question of the extrusion of man from physics. The physicist has extruded him in order to avoid adding subjective ingredients to his system, while students of human phenomena have done so because they could not abide the idea of physical phenomena influencing the mental. To harbor both in one system is to Köhler not only proper but essential for scientific purposes. His own view is that physics itself is the work of man and it is through him that inevitable connection exists between the phenomenal and natural worlds. Man as a link between the two is, for example, illustrated by the fact that human motives are "identified with physical tensions which develop between the cortical correlate of the self and that of an object" (p. 377).

In appraising this volume we cannot laud too highly the investigation by a scientist of such important phenomena as values. It is a distinctive merit to divest oneself of the conventional phobia for everything which is not clearly a set of far-fetched abstractions. Too often scientists confuse the fact that science must attain to abstractions with the idea that they must exclusively occupy themselves with abstractions. But still more praiseworthy is Köhler's attitude of studying values on a scientific level. In so far as he carries out this intention he avoids the paradox of recognizing the importance of values and then interpreting them as sentimental and mystical entities really unrelated to natural phenomena.

Unfortunately, the author heavily weights the debit side of the balance. His actual performance does not square well with his intentions. Though he aims to accord values a definite place in nature, he, after all, treats them as tenuous abstractions because for him nature itself turns out to be a set of abstractions. In his final analysis nature becomes reduced to the constructions of physics, while the objects and processes with which the scientist begins and which set him his problems are converted into phenomena in the sense of subjective states and qualities. In consequence, Köhler finds himself dealing with different kinds of worlds. True enough, he wants to keep the number of such worlds down to two, that of physics and that of subjective states, and for this purpose adopts Occam's razor in the form that "worlds ought not to be multiplied beyond strict necessity" (p. 236). He does not abide by the rule, however, in view of the fact that nature is, and science knows, only one world. All the rest is phantastic and unnecessary construction.

Basic to Köhler's unacceptable treatment of both facts and values is his inadequate conception of scientific construction, which he limits to the work of the physicist. That the subjective or introspective states or qualities are likewise constructions he does not appear to consider. In fact, Köhler definitely accepts such inadequate and invalid constructions as psychic states and assumes that they are crude data or the given of science. It may be suggested that the crude data of science are always the concrete phenomena of everyday life which initiate all scientific activity. Scientific work begins by an actual interbehavior with such phenomena, and its results may be summed up as: (1) the construction of descriptions and explanations on the basis of such interbehavior; (2) the discovery, by means of such contacts with the original data, of other, at first unsuspected, crude data; and (3) the testing of such constructions by ascertaining whether they comport with the phenomena of the original contacts.

It is only Köhler's unsatisfactory conception of science and its work that prevents him from treating values as definite phenomena in human situations. When we assume that science operates in a domain of contacts with concrete phenomena, the scientist has a larger range of activities than that circumscribed by conceptual physics. In this larger domain the student of values does not face the problem of how values can be assimilated to a world of physical abstractions. Consequently, no heroic measures need be adopted to prove that values are factors of a cosmic dynamics or that the dynamic world harbors objective strivings. In other words, Köhler's quixotic measures are supererogatory devices made necessary by accepting a traditional dualism such as the scientists of the Seventeenth Century established.

Not only does Köhler's dualism interfere with an adequate treatment of values; it likewise results in a flagrant misinterpretation of facts. For example, when Köhler attempts to establish his resemblance between the phenomenal and the physical, he does so by means of the brain. It is a serious predicament to require such functions of the brain as to misconstrue completely its nature and operation. Is the brain anything other than a definite biological mechanism for the coördination and integration of the activities of the organism? To make it the mediator between physical and psychic states is to go completely counter to the teachings of biological science.

A curious paradox issues from Köhler's use of the brain as a metaphysical device; namely, a thorough contradiction of what the reviewer, among others, regards as the primary contribution of Gestalt psychology. It is no small addition to psychological theory to interpret a psychological phenomenon as a concrete interactional relationship between an organism on the one hand and an object on the other. For example, when two unequal parallel lines are presented, the subject responds to them as though they were parts of a trapezoid or a rectangle, as a function of the distance between them. Now, instead of dealing with these situations as two objective relations between activities of individuals and line configurations, Köhler prefers to use the brain as a mediator between phenomenal or psychic geometry and physical gradients. Resort to the brain hypothesis reduces psychological description entirely to internal principles in the organism.

Does not Köhler thus employ the culturally established dualistic tradition to misinterpret experiments in order to establish values among facts? In view of neurophysiological findings it turns out that interpreting experiments cortically not only does not establish values, but rather casts suspicion upon Köhler's notion of values. How can such values aid us in dealing with the insistent problems of human affairs?

And finally, it is significant to what extremes the author resorts in his argument for dualism and its cortical intermediary. First, he regards the objection to the brain explanation as emotional bias on the part of the objectors. He counsels courage to adopt a dualistic instead of a materialistic-monistic metaphysics without considering the advisability of rejecting traditional metaphysics altogether. His own courage is morally supported by calling neural action psychophysical and distinguishing between the "body" as percept and the organism as a transphenomenal entity (p. 187). In the second place, he employs the hypothesis argument: In order to account for certain phenomena it is necessary to introduce the brain hypothesis even though "no such traces of neural events can yet be demonstrated by neurological methods" (p. 237). In support of his view he suggests that the atomic theory was rejected in the Eighteenth and Nineteenth Centuries for lack of evidence and that when direct evidence is available it is no longer an hypothesis. The obvious reply, of course, is that scientists make mistakes and that to accept any proposition as an hypothesis is to make way for every kind of occultism on the ground that some day it may be established. This was not true, by the way, for the caloric, phlogiston, microzyme, and gemmule hypotheses, among thousands of others. Moreover, in the case of the neural hypothesis we know its origin in historical tradition, and certainly the accumulated knowledge of neurology increasingly shows the exclusively biological and nonpsychical character of the brain.

It is a source of regret to the reviewer that he finds it necessary so adversely to criticize such an admirably motivated and learned a book. In extenuation it may be said that it is precisely the necessity to establish values in a world of facts that requires us to obtain a proper orientation toward this most important and elusive problem.

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Beck, S. J. Personality structure in schizophrenia: a Rorschach investigation in 81 patients and 64 controls. New York: Nervous & Mental Disease Publishing Co., 1938. Pp. ix+88.

This monograph attacks the problem of the Rorschach test in schizophrenia from various sides. For purposes of demonstration, a representative record each of a normal and of a schizophrenic person is given in full with their scores, the latter being subjected to a detailed analysis and psychological interpretation. Except for this example, the material is dealt with in a statistical way, *i.e.* the numerical data for the various test categories in the schizophrenic group are scrutinized and paralleled with corresponding data from the nonpsychotic group. Finally, the psychological implications of the findings are discussed at some length.

Everyone who has had some experience with schizophrenic "Rorschachs" is acquainted with the wide variety of reaction forms that one finds among these patients. It is therefore rather unfortunate that only one such record is reproduced in its entirety, especially in a monograph devoted to the subject. The presented case is well chosen, however. It brings out as many features characteristic of the disease group as can be expected in a single record, particularly since it is compared with the responses of a control subject of the same sex, educational level, and of similar age. To beginners in the field, this case analysis may be warmly recommended. The analysis begins with a careful consideration of the psychological significance of the numerical values of each test category. A very valuable discussion of the relationships among these categories follows. The reader is warned against interpretations made on the basis of single test findings taken out of the context of the whole record. By means of a diagram the interdependence, or inner balance, of the major categories is well illustrated. One regrets, however, that the analysis of the individual record has not been carried further into the finer, more qualitative aspects of the test, such as one finds in the well-known *Rorschach-Oberholzer* study or in some publications of the *Rorschach Research Exchange.*

The statistical section is the best. Means, medians, Q's, S.D.'s, ogives, and critical ratios are utilized to arrive at the following list of test categories, as differentiating the two groups in a statistically significant way: Dr, DW, C, CF, FC, F+, and P. M, in contrast to Rorschach's contention, is not higher in schizophrenics than in normals. Unfortunately, original responses, which have proven a useful differentiating criterion, are not considered by the author. On the whole, however, this examination of the various test categories lends gratifying corroboration to Rorschach's own, Skalweit's, and the reviewer's findings in schizophrenic patients. Important, too, is the stress on the great variability within the schizophrenic group, well demonstrated by a histogram. Particularly illustrative is the diagram representing the relationship of color to movement.

The main ideas of the section dealing with the structure in personality are familiar to the reader of the author's earlier publications, especially of his book, *Introduction to the Rorschach method: a manual of personality study* (1937). The whole personality is thought of as consisting of a psychological and of a social personality. The former refers to "certain major psychological processes . . . certain traits adding to finer lineament . . ."; the latter is "the coloring which the individual has taken on by reason of his personal experience . . . the ideas and special reference to his wishes . . ." Differences in personality structure are conceived chiefly in terms of balance between the major psychological processes, as yielded by the Rorschach test. Thus, in a superior normal the main personality factors are all present in high quantities. In a person of lower, but healthy, endowment all the processes are reduced proportionately, still preserving the balance. The abnormal person, on the other hand, is characterized by some imbalance in his structure. The discussion culminates in some rather vague speculations as to whether personality patterns are limited in number and how accurate personality measurement can be. After this general treatment the statistical findings on the schizophrenic group are taken up again in order to construct a sketch of the schizophrenic personality in terms of the above principles.

Besides the parts of the book discussed here a description of the Rorschach method and a bibliographical note are included.

In summary, one might say that this should be a useful book. One wishes, however, that the author had used some of his broad experience in bringing out more fully the significance of the total individual record in all its quantitative and qualitative potentialities.

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VAN DER HOOP, J. H. Bewusstseinstypen und ihre Beziehung zur Psychopathologie. Bern: Hans Huber, 1937. Pp. 375.

In this book Van der Hoop continues his work of modifying and applying Jung's typological system. The first principle of classification is still that of extraversion-introversion. Psychiatric experience and the use of Husserl's phenomenological procedure have led Van der Hoop to make changes in the four mental functions which provide the second principle of classification. Jung's sensation type has been replaced by the instinctive type, " instinctive " being used almost in its popular sense: natural-feeling, unpremeditated, impulsive. The instinctive function includes direct emotional reactions and direct impulses to overt behavior; as described in detail, it is calculated to fit a large number of people who are so natural and sometimes so ordinary that they are likely to elude a typological classification. The feeling function has been broadened to include sentiment (the concept offered by McDougall and Shand), as well as affectivity and mood. The functions of intuition and thought, and the corresponding types, are more nearly the same as Jung's. There is presumably a native disposition which determines the dominance of a particular function and the appearance of the resulting personality type. However, the classification of a given person may be complicated in two ways (among others): by the presence of a second one of the four functions in lesser degree; and by " polar " action, the contradictory action of a function which is incompatible with the principal function.

Van der Hoop uses explanation in terms of personality type to supplement, rather than to replace, explanation in terms of temperament and the Freudian mechanisms. If a given person is to develop a neurosis, his personality type may determine what form the neurosis will take—Freud's problem of the *Neurosenwahl*. The typology is especially useful in understanding constitutional psychopathic personalities, which appear as caricatures of the normal personality types. There are also interesting applications of typology to the problems of mutual understanding and the psychology of psychologists. Topics in epistemology, developmental theory, and the psychology of religion are handled with distinctly less success.

It is encouraging to find an honest and serious typological work, especially in view of the incredible perversions of typology, inspired by racial prejudice, which have appeared elsewhere. Some of Van der Hoop's type sketches are so convincing that they suggest certain actual contemporary people, and include the most puzzling features of these people. It might very well be that by means both subtle and rigorous one could confirm some of the assertions of this typology. But its systematic concepts are taken chiefly from a moribund-act psychology and are hardly quickened into life by the use of phenomenology. The basic difficulty is one of nonoperational definition. We know very little of the operations which lead to introspective observation of the larger features of experience, and so cannot take advantage of phenomenology or its statements. The direct "intuitive" judgment of other people is equally resistant to operational analysis and equally unsatisfactory as an unaided source of scientific psychology, whatever may be its importance in everyday life. If these operational difficulties are widely appreciated, a typology founded on phenomenology and personal observation will have hard sledding.

JOHN VOLKMANN.

Columbia University.

VILLEY, G. La psychiatrie et les sciences de l'homme: essai de synthèse scientifique. Paris: Félix Alcan, 1938. Pp. 194.

This essay, which is addressed to both the profession and the educated public, has three aims. It attempts to introduce psychiatry as a science, to show how psychiatry is related to medicine and the other sciences, and to establish the author's views on nosology and etiology. This is a big order for less than 200 pages and the result is a very uneven hodgepodge. The educated layman will gain nothing from the technically difficult and methodologically confused sections on psychological language and nosology; the professional psychiatrist will be bored stiff by the sketchy discussions of history, of the scope of psychiatry and its relations to the other sciences, and of hospital care and management. It is unfortunate that the author chose to combine his original theses in a book which otherwise might be considered an eloquent and modern plea for psychiatry to the educated Frenchman.

The author's personal psychiatric contributions are not worth much consideration. He follows the ideas of Chaslin on psychological language and of Janet on nervous equilibrium and attempts a synthesis of these viewpoints. Both of these ideas are good as far as they go, but they never have gone very far, and the reviewer is unable to see that the author has advanced with them. There is no indication that Villey is familiar with either modern American or German psychiatric theory. A few references to Freud indicate his totally inadequate conception of psychoanalysis. The sociological theses occasionally advanced are hopelessly outdated and naïve. There are quite a few moral and ethical evaluations which do not belong to modern psychiatry. The book contains a short and very weighted bibliography but no index.

J. F. Brown.

University of Kansas.

Roberts, H. The troubled mind: a general account of the human mind, and its disorders and their remedies. New York: Dutton, 1939. Pp. iv+284.

Fletcher, P. Life without fear. New York: Dutton, 1939. Pp. 111.

These two books are, from internal evidence, of British origin, reprinted for popular sale in this country. Dr. Roberts' book, with several chapters on the insanities by Dr. Margaret N. Jackson, is an entertainingly written, middle-of-the-way account of human psychology, especially with respect to the problems of adjustment. The author's psychology leans rather heavily upon innate traits and on a limited psychoanalysis. While of little interest to the professional psychologist or psychiatrist, it will probably be appreciated by intelligent laymen. Dr. Jackson's chapters include very brief, conventional descriptions of the common types of mental disorders.

Fletcher's little book emphasizes fear as the basis of maladjustment, and the point of view is sufficiently given in one quotation: ". . . I have reached the conclusion that no method of therapy will effect permanent and constructive changes in human behaviour except to the extent that it is based upon a realization of the spiritual nature of man, and reverence for personality as the instrument of God for the expression of His nature and the fulfillment of His will."

C. M. Louttit.

Indiana University.

Walton, R. P. Marihuana: America's new drug problem. Philadelphia: Lippincott, 1938. Pp. ix+223.

The book summarizes what is known of the use of this drug and its kindred forms, such as hashish, all derived from the hemp plant. Outlined in separate chapters are the history of the hashish vice—which is very ancient, its present distribution throughout the world, and the present status of the marihuana vice in the United States, this latter contributed by Frank R. Gomila and Madeline C. Gomila of New Orleans. The use of this drug in some form is very widespread throughout the world and has increased rapidly in the United States and Canada during the last few years.

One difficulty with the suppression of the vice is the ease with which the drug may be procured. The hemp plant is easily obtained inasmuch as its fibers are of such great economic value. The plant adapts itself easily to almost any habitat and can be successfully grown almost anywhere in the civilized world and in favorable habitats actually survives as a weed. Also, it is easily administered by ingestion or by smoking, and it is not necessary to prepare or purify those parts of the plant which are used.

Three chapters are given to descriptions of the hashish experience from the subjective, objective, and psychiatric points of approach. Subjectively, the subject experiences a euphoria and either a complete anesthesia or at least a hypoesthesia. Many of the effects are similar to those described for peyote (mescaline). Some space is given to the chronic effects of the drug as well as the acute effects. The remaining chapters treat therapeutic applications, pharmaceutical and chemical considerations both of the drug and of the active principle and nomenclature. There is a bibliography of 419 titles.

The book cannot fail to be of interest to psychologists working in many different fields. It must be of interest to the psychologist who is interested in abnormal psychology and the psychology of drugs and also to those interested in psychiatric problems. It must also be of great interest to the psychologist who is interested in social psychology because it presents a problem of social change of recent date in the United States—so that the beginnings may still be available—and a social change which the authors show to be of very considerable magnitude. In this regard, the authors point out that many tons of hemp have been grown for fiber for a great many years in the United States without any considerable report of drug addiction. Hence, the availability of information and of the drug itself are not the only factors in its use in proportions such as those existing at present. The authors point out that at least two other factors must be present. The first is a "reasonably intimate social contact between an uninitiated population and a population which practices the vice; the other is the temperament and social conditions of the people who adopt the practice."

SAMUEL W. FERNBERGER.

University of Pennsylvania.

FRITZ, M. F. Collection and presentation of statistical data in psychology and education. New York: Prentice-Hall, 1939. Pp. vi+58.

The purpose of this booklet is clearly stated in the opening paragraph of the Preface:

> "This manual attempts to present in a very simple fashion for the beginner some of the procedures in the collection, tabulation, and graphic presentation of statistical materials. It will not, of course, replace the regular textbooks on statistics or tests and measurements, but is intended to serve as a supplement. Its use will enable the student to answer certain questions for himself, thus conserving class time for other matters. Although many of the things considered may seem trivial to the trained person, the beginner finds these items a source of considerable perplexity. Therefore, no apology will be offered for the simplicity of the presentation."

This aim is fulfilled in the pages which follow; no statistical formulae are to be found, and the language and illustrations are simple and to the point. In this connection it might be noted that a part of the material was originally written as an aid in the training of workers for a state re-employment service.

The manual is divided into forty-nine sections which include the following topics: "1. Why you should be interested in statistics"; "7. How accurate do we need to be?"; "9. Use of commas"; "12. Types of error in collecting data"; "23. Use of questionnaires"; "35. How to make graphic illustrations"; "37. Smoothing curves"; "40. The use of percentages"; "43. Label everything"; "44. Check all calculations."

A list of twenty-five discussion questions is provided to clarify further the points brought out in the text. Twenty-four books on statistics "which emphasize practical applications rather than theory" (p. 53) and an index are appended.

The pages of this booklet contain an admirable simplification of subject matter. Teachers of courses in which an introduction to the collection and presentation of educational and psychological data is a part of the work should find that this manual will fill a real need.

R. S. SACKETT.

The American University.

BOOKS RECEIVED

Freeman, E. Principles of general psychology. New York: Holt, 1939. Pp. xiv+530.

Gesell, A., Amatruda, C. S., Castner, B. M., & Thompson, H. Biographies of child development: the mental growth careers of eighty-four infants and children. New York: Hoeber, 1939. Pp. xvii+328.

Griffith, C. R. Psychology applied to teaching and learning. New York: Farrar & Rinehart, 1939. Pp. xiii+650.

Hoopes, G. G. Out of the running. Springfield, Ill.: Thomas, 1939. Pp. xvii+158.

Latour, M. Premiers principes d'une théorie générale des émotions. (Nouvelle édition revue et augmentée: Observations complémentaires. Treizième série.) Interprétation des faits d'aliénation selon la théorie générale. Paris: Félix Alcan, 108, Boulevard Saint-Germain, VI^{e}, 1938. Pp. 55.

Link, H. C. The rediscovery of man. New York: Macmillan, 1938. Pp. xi+257.

Moore, H. Psychology for business and industry. New York: McGraw-Hill, 1939. Pp. xi+527.

Skodak, M. Children in foster homes: a study in mental development. *Univ. Ia Stud. Child Welf.*, Vol. XVI, No. 1. Iowa City: University, 1939. Pp. 156.

NOTES AND NEWS

Dr. Walter Dill Scott, since 1920 president of Northwestern University, has presented his resignation to the trustees. He expressed the hope that his successor might be elected so as to permit his retirement in the autumn. Before becoming president, Dr. Scott had been connected with the university since 1900, successively as instructor, assistant professor and professor of psychology and director of the psychological laboratory.—*Science.*

Erik Homburger Erikson, formerly assistant professor in the Yale Medical School, has accepted an appointment as research associate in the Institute of Child Welfare, University of California.

A new Bureau of Instructional Research has recently been created at the University of Nebraska with J. P. Guilford, professor of psychology, as director. Professor Guilford will continue to devote part of his time to teaching. Mr. H. M. Cox of the University of Georgia has been appointed to full-time work in the Bureau.

The Midwestern Psychological Association will hold its annual meeting at the University of Nebraska, Lincoln, Nebraska, on May 5 and 6, 1939, under the presidency of Dr. E. S. Conklin of Indiana University. The title of Dr. Conklin's presidential address will be "The Status of Academic Psychology." A special feature of the meeting will be the commemoration of the fiftieth anniversary of the founding of the psychological laboratory at the University of Nebraska—one of America's earliest—by Harry Kirke Wolfe.

The Washington-Baltimore branch of the American Psychological Association held its second meeting of the year at the Waverly Press in Baltimore on January 4. The following program was presented:

G. Wilson Shaffer: "Suggestibility in Relation to Diagnostic Types."
H. C. Smith: "Variations in Color-Matching Ability."
Vernon Scheidt: "Some Industrial Applications of Psychology."

At the recent Lima Conference a resolution was passed on behalf of the Eighth Biennial Congress of the World Federation of Education Associations to be held at Rio de Janeiro August 6–11, 1939. In essence, the resolution was to bring this Congress to the attention of educational authorities and teachers in the American States and direct the attention of delegates to the opportunity of visiting educational institutions in other countries en route. Recognition of " the important rôle played by teachers and supervisors in the development in youth of appreciation of international understanding and good will " and the unusual opportunity afforded by this Congress to gain such knowledge and understanding prompted the resolution.

The " Rotterdam " is to sail from New York on July 5 and from New Orleans on July 10 and will provide facilities for those attending the Congress. Further particulars may be obtained by writing to headquarters of the Association at 1201 Sixteenth Street, N.W., Washington, D. C.

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Psychological Bulletin

THE SOCIAL PSYCHOLOGY OF THE VERTEBRATES

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Yale Laboratories of Primate Biology

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INTRODUCTION

In limiting the field covered by this review the term *social psychology* is defined as the study of the interactive behavior of organisms as distinguished from *sociology*, which is concerned with the behavior of groups as wholes. The experimental beginnings of the social psychology of the vertebrates will be reviewed in this paper. Almost all of the strictly experimental studies have been made during the last decade, and toward these primary attention will be directed. A number of laboratory observations and a few field studies must be included to complete the survey of the available literature relevant to this new area of study. A summary of the vast literature on the natural history of the vertebrates is obviously outside the scope of the report. Of the laboratory studies which have some bearing on social interaction, those on sex physiology and behavior are arbitrarily excluded. It is unnecessary to point out that a complete theoretical consideration of the social psychology of the vertebrates could not be made without reference to reproductive behavior.

The material to be presented has been classified according to various aspects of social behavior. These topics are regarded as convenient rather than correlative. Within each division reports on different kinds of vertebrates are grouped together, and a zoological classification[1] of the literature is offered by the arrangement of the bibliography. The majority of the studies are exploratory in nature, projected in various directions on diverse types of vertebrates. Individual review of method and content of each is therefore preferable to an attempted summary statement of the findings from the inceptive stages of this work.

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[1] Division has been made according to the classes of vertebrates, except that the primates have always been given separate treatment. The term *mammals* will always be understood by the reader as excluding primates. The term *primates* refers throughout to infrahuman primates.

Mention should be made of a number of references which include a wider range of material than is given here or which are intensive reviews of particular groups of vertebrates. A general survey of animal sociology, which places chief emphasis on descriptions of types of associations adapted to various sexual relationships, is given in Alverdes' book (**160**). Reference is made therein to the early works of Espinas, Ziegler, and Doflein, and much material is taken from Brehm's *Tierleben.* Allee (**156**) also summarizes the earlier work and offers a simplification of Deegener's (**164**) comprehensive classification of the social organization of animals. Allee introduces the experimental study of aggregations, particularly as they appear in lower organisms. He elucidates the principle of "co-operation," or automatic interdependence, among organisms, ranging from a supplementation of foodstuffs and waste products to an intraspecies teamwork. His later reviews (**157**, **158**, **159**) survey some of the same material and extend consideration to the higher forms, although they do not pretend to be exhaustive treatments of the literature. Recent books by Picard (**172**) and Rabaud (**173**) are cited as exemplifying different theoretical points of view. Special topics relative to the social behavior of animals are treated in Groos's work on play (**167**) and in Kropotkin's on mutual aid (**170**).

Articles expressing the need for a comparative social psychology are offered by Eidmann (**165**) and Maslow (**171**), while textbook discussions of the subject are given by Tinklepaugh (**174**), Warden, Jenkins, and Warner (**175**, **176**), and Katz (**169**), the last of which is noteworthy for an adequate summary of recent German work.

The social behavior of various groups of vertebrates has been given special treatment in recent biological texts (**5**, **27**) and semi-popular books (**19**, **44**). The literature on anthropoid apes, covering a period from 1912 through 1925, is reviewed by Yerkes and Child (**150**). In *The great apes,* Yerkes and Yerkes (**153**) discuss all the literature available on these primates through 1929. Throughout the present paper reference will rarely be made to work included in this book. Zuckerman (**155**) covers the most important literature on primate social behavior in his *Social life of monkeys and apes.* Finally, 3 chapters in the *Handbook of social psychology* are of particular interest: Friedmann (**47**) and Alverdes (**93**) discuss, respectively, the group behavior of birds and mammals, using field observations as chief source material. Yerkes and Yerkes (**154**) present an admirable chapter on the social life of 4 representative primates—the lemur, howler monkey, baboon, and chimpanzee, in which the field studies of Nissen (**137**) and Carpenter (**115**) are summarized in detail. To these field studies reference will be made in this paper only for special topics. The Yerkes also offer a constructive outline of areas for study in developing the experimental social psychology of primates.

STIMULUS-RESPONSE ASPECTS OF AGGREGATIONAL BEHAVIOR IN LOWER VERTEBRATES

The social grouping found among lower vertebrates seems to be limited, save for mating groups, to loose associations in which little or no internal organization appears. For these Allee (**156**) has adopted the term *aggregations*. Aggregations have been regarded with renewed interest since the publication of Allee's book, which treats the various physical and behavioral factors that influence their formation. It may be noted in passing that the physiology of this kind of group behavior is of particular interest to the psychologist in suggesting fundamental drive mechanisms which may come to motivate social interaction during the course of phylogeny or ontogeny.

Fish. Most fish form aggregations at various times, particularly under adverse conditions. A special type of such behavior has been recognized in schooling, which Parr (**9**) defined as swimming together in roughly parallel courses in closely bunched groups. He distinguished between temporary or unstable schools, formed when some outside factor disturbs the fish, and stable schools, which are difficult to disperse and which seem to depend on a strong mutual attraction between individuals. Parr offered a theory of schooling behavior based on experiments showing that blinded fish do not school, in which he postulated a positive attraction between individuals which brings them within a certain range of each other, and a repellent stimulus condition arising from sensations of too close visual accommodation. Balance of stimuli from other individuals on either side tends to keep a fish on a straight course, while individuals on the outside of the school tend to be slightly oriented toward the center of the group. The theory of mutual attraction was also used to account for milling, which occurs when members of a school turn and swim about in concentric circles. In a later paper, Parr (**10**) recognized sex behavior as a reaction somewhat opposed to schooling. In this connection a recent review by Noble (**8**) of the literature on sexual dimorphism and sexual selection among fishes should be mentioned.

Papers have recently appeared which tend to substantiate points in Parr's theory. Bowen (**1**, **2**) demonstrated the primary importance of vision in catfish aggregations and the secondary use of tactual and vibratory sensitivity when some of the fish were blinded. Breder (**3**) found that certain fish would not approach a solid object nearer than a particular point, "which is probably the limit of visibility." From experiments on the effects of changes in temperature, chemical composition of water, and amount of light, Breder and Nigrelli (**4**) concluded that schooling is motivated by a "primary impulse" dependent on vision and inhibited by specific requirements, as feeding and reproduction. Mutual orientation in apparent preference to photic orientation was studied by Langlois (**6**) in bass. Spooner (**13**) found that members of this species were attracted to one another through a glass partition, or that a mirror image served as well. He also found that neither the complete form nor the movement of another fish was required to stimulate the approaching response. The course of schools taken in avoiding a projecting wall has been described in trigonometric symbols for a variety of fish by Tauti and co-workers (**14**, **15**, **16**).

Reptiles and Amphibia. The aggregation behavior of the small brown snake *(Storeria dekayi)* has been carefully studied in the laboratory by Noble and Clausen (**29**). Temperature and humidity conditions promoting aggregation were experimentally determined, and the use of vision as the chief sense modality, with olfaction as a secondary mode, was demonstrated. The snakes were attracted to one another by movement rather than form or color, and a mirror image was found effective as a stimulus for approach.

Concerning the social stimuli involved in the aggregations (chiefly for mating) of amphibia there seems to be no experimental evidence beyond the importance of vocalization in frogs (**27**). Aggregational behavior is also found among the higher vertebrates during seasons of breeding or migration and when influenced by various adverse physical conditions. The psychological nature of this behavior has not been studied in the laboratory, probably because the attention of experimenters has been directed toward more complex forms of social behavior.

SOCIAL FACILITATION

The general term *social facilitation* may refer to any increment of individual activity which results from the presence of another individual, and can be regarded as one of the most basic forms of social interaction. In the literature on animal behavior its meaning has been restricted to increments in the frequency and intensity of responses already learned by an individual, shown in the presence of other individuals usually engaged in the same behavior. Imitation, on the other hand, has referred to observational learning in problem situations. While social facilitation is manifestly involved in observational learning, it has been convenient in this review to distinguish between studies on social facilitation and imitation. No real dichotomy between them is implied. Since almost all of the former are concerned primarily with social factors (size of groups, dominance status of individuals, etc.), they will be given more detailed treatment than most of the imitation studies where chief emphasis is placed on the learning process.

Fish. Schuett (**11**) observed goldfish in groups of 1, 4, 8, and 16 individuals and found that amount of bodily activity was related, within limits, to size of group, indicating a possible factor of social facilitation. Relative oxygen consumption was controlled.

Welty (**17**) performed a careful series of experiments on 930 fish which demonstrated social facilitation of unlearned acts and of the simultaneous learning of simple habits by groups of fish. The subjects were chiefly goldfish, but also included pussy minnows, killifish, lake shiners, paradise fish, and zebra fish. The general methodology of the 33 experiments required a rectangular aquarium containing 1 or more wire screens, each having a hole or door in it, through which fish were trained to swim into another compartment to receive food. The chief measure of learning was the decrease in time of movement from one compartment to another. Four experiments showed that fish in groups of 2, 4, and 8 learned a single-door aquarium maze more quickly than individual fish. Four more experiments employed comparable groups of 4 individuals in 15 aquaria; to some groups a trained fish was added in the starting compartment to serve as a " leader "; with other groups an untrained fish was placed in the goal compartment to serve as a " lure "; and no other fish were added to the control groups. Results showed most rapid learning for the groups with leaders, and those with the lure fish learned faster than controls.

Unique tests of " group cohesion " were made in 6 experiments in which groups of 4 fish were placed in the central compartments of aquaria. All members of some groups were trained to swim to one compartment, while 2 members of other groups were trained to go to the forward compartment and the other 2 to the rear compartment. Most rapid learning took place when all fish moved in the same direction. Carefully controlled experiments showed that fish consumed more food (the small crustacean *Daphnia*) when in groups of 4 than when alone. The "confusion effect," resulting from apparent inhibition of the feeding response when too many *Daphnia* were available to a single animal, may have played a large part in the results, since, when *Daphnia* were given to individual fish in groups of 100 instead of 600, they consumed as much per day as the fish in groups. In general, the results of Welty's series of well-controlled experiments leave little doubt of the susceptibility of fish to social facilitation.

Birds. Breed (**36**) made incidental observations on apparent social facilitation of drinking, and perhaps also of pecking, in the chick, and Craig (**41**) found evidence for facilitation of pecking, but not drinking, in the dove. Fletcher, Cowan, and Arlitt (**46**) found that the pecking response developed as rapidly with 4 isolated chicks as with normal chicks, therefore concluding that social facilitation was unimportant. In 1927 Fischel (**45**) made observations on a flock of 500 white leghorn hens, most of which will be treated under another topic. Social facilitation of feeding was indicated in the observation that when one hen began to eat, others, who had shown no signs of hunger previously, ran toward this one and began feeding.

Part of the evidence used by Bayer (**34**) for a 2-component theory of hunger was obtained from observations on social facilitation of feeding in hens. Using 8 hens, whose dominance hierarchy had been determined, he allowed 1 to eat at a pile of grain until apparently satiated, and then introduced a hungry hen. The satiated animal began, in every case, to eat again, consuming between 25% and 43% of the amount of food originally eaten, or an average of 34% for the group. If the satiated animal were dominant over the invader, she would attempt to prevent her from eating by pecking at her, finally beginning to eat again herself, when her pecking proved ineffective; if subordinate, she would eat again despite the pecks received from the dominant, hungry animal. Greater increments in eating were shown by a satiated animal when 3 hungry hens were introduced in place of 1, averaging 53% and ranging from 33% to 67%. Use of 3 satiated and 1 hungry animal resulted in considerably less increment of feeding in satiated animals. A fourth experiment showed that, when 4 hens ate alone and together on alternate days, they showed social increments between 33% and 200%, averaging 96%.

Allee and Masure (**32**) studied the effect of the presence of one shell parrakeet (lovebird) upon the learning of a color discrimination problem by another. A total of 97 birds, 4 months old, were used until they were well past the point of sexual maturation. Birds trained in pairs learned more slowly and showed longer response times and greater variability than individual learners, regardless of the sex of the partners. Birds caged in pairs and trained alone reacted as did singly caged birds. When mastery was reached, individuals changed from the isolated to paired routine and vice versa gave performances identical with those trained under paired or isolated conditions. Mutual interference, rather than social facilitation, characterized the results; the performance of the pair approximated that of the poorer learner.

Mammals. In a number of experiments on rats Harlow (**99**) clearly demonstrated the effect of social facilitation on feeding. In the best controlled experiment of the series, 34 animals, weaned at 18 days, were reared in individual cages until 30 days of age. They were then paired on the basis of weight and were fed alternately in pairs and individually for 20 days. In the social situation 8% more food was eaten than in the individual, which difference was found statistically reliable. No change in amount of facilitation with practice was detected. Other experiments indicated that experience was not important to social facilitation, since rats, fed only on milk, showed the social increment when paired and fed solid food for the first time. Also, rats who were reared and fed in isolation showed facilitation on the first experimental period. Competition characterized the behavior of paired animals where social facilitation was observed; when one partner was restrained and could not compete, no increment was shown in the other's behavior. A technique adopted from Bayer, in which a rat already fed for an hour was joined by a hungry animal, failed to increase the former's eating over what would have been eaten during a second hour had it been left alone. Harlow's discovery that facilitation scores were no greater with groups of 3 than groups of 2 rats differed from the findings of Bayer with the hen.

Studies by Lepley (**103**) and Bruce (**96**) indicated social facilitation of running in rats, while Waters (**108**) found no evidence for facilitation of maze learning. Lepley gave individuals of 2 groups, of 6 littermates each, 20 preliminary runs, and 80 untimed runs in a 30-foot runway, after which 40 timed runs were allowed, and pairings were made on the basis of speed. Each pair was given 40 timed runs to the goal box, where each animal received food. Animals in pairs ran slightly faster than when alone. A competitive situation was arranged for the following 160 runs, in which only the winning animal was rewarded. Losers of early trials seldom won later, except in a few trials at the very end. Bruce's results were obtained when paired rats were allowed to run a free pathway to a drinking fountain. An increase in running speed and amount of water drunk followed an initial decrease in pairs of experienced rats. Pairs of naïve rats showed initial increase of speed, while those paired with experienced animals ran faster, covered less distance, drank more water, and took less time for the first drink than naïve rats tested alone. Waters employed 2 sets of 2 matched groups of 20 rats each on 2 forms (elevated and alley) of the Miles unidirectional maze, members of one group running one maze singly and another in pairs. Median errors and time per run failed to differentiate results from paired and isolated animals.

Primates. Social facilitation of feeding was studied in 3 pairs of 6 rhesus monkeys by Harlow and Yudin (127). In the 4 experimental situations employed, there were placed in an observation chamber (1) a cage containing both animals and the food pan (direct competition); (2) 2 cages, 1 foot apart, with 1 animal and a food pan in each (noncompetitive); (3) 2 cages, 1 foot apart, with 1 animal and a food pan in each and an additional food pan between the cages available to each subject (combined competitive and noncompetitive); and (4) 2 cages placed next to one another so that the monkey in each might reach into the food pan in the other cage ("robber" situation). Tests with the 3 pairs of monkeys extended over 10 days in each situation, odd days serving as controls. When orange was used, the situations yielded percentages of social facilitation in the order listed above: (1) 13%, (2) 23%, (3) 53%, and (4) 69%, although all subjects did not always show the effect. When a less preferred food—bread—was used in situation (1) the percentage of facilitation was 152. Harlow suggested that since orange was greatly preferred to bread [subjects ate 962 pieces of orange and 75 of bread during the control periods of situation (1)], it was "liked" and eaten without need of a secondary stimulus. The qualitative data did not indicate that any clear attitudes of dominance or subordination were manifested between members of the pair.

These data are comparable to those of the same author on the rat and suggest how much more susceptible the monkey is to facilitation, even when competition is eliminated. Observations on feeding behavior of other pairs of monkeys reported in this paper, which indicate "envy" in Bayer's terminology, will be cited in the section on dominance.

Yerkes' (**148**) study of extraspecies suggestibility in the chimpanzee may be considered under the general topic of social facilitation of feeding, although here the social stimulus was the experimenter. Twenty-one animals, 4 males and 17 females, ranging in age from infancy to mid-maturity, were offered pieces of filter paper on 4 successive days. The extent to which they accepted it as foodstuff was rated on a 5-point scale ranging from (0) refusal to accept or prompt rejection without carrying it to lips to (4) eager acceptance, chewing, and swallowing with evident satisfaction. Average scores for the 4 preliminary trials were 36, 23, 26, and 20. On experimental days, as the filter paper was given the animal, the experimenter simultaneously took a piece, drew it into his mouth, and chewed it noisily. Scores for these days were 18, 27, 27, 24. A trend toward extinction of the eating response was thus arrested by the experimental procedure, indicating the operation of social facilitation from the experimenter.

IMITATION

By far the greatest number of experimental studies of imitation were made between the years 1900 and 1913, when interest in the process as a demonstration of animal intelligence, as well as a unique mode of problem solution, was at its height. Some 20 studies from this period are adequately summarized in Watson's (**177**) chapter on "The Abridgement of the Learning Process" (pp. 277–296). A more recent review of imitation in the vertebrates is given by Warden, Jenkins, and Warner (**176**), and by Spence (**140**) for the primates. Only the most recent studies will be treated here. Attention is called to Ball's recent report (**112**) of a case of apparent imitation in a monkey who lived with a cat and drank in typical cat fashion.

Fish. Welty's (**17**) work on fish included 4 experiments in which a single-unit maze was placed in the wider compartment of a longitudinally divided rectangular aquarium, and a fish was allowed to observe, from the narrower compartment, another fish swimming through the maze 10 or 20 times. Tested 1 hour later, the observing fish swam the maze faster than controls who were prevented from seeing the maze by an opaque screen. A special checking procedure, in which food was presented in the goal compartment at intervals, but in which no demonstrating animal performed, indicated that a real facilitation of the learning process, or a type of imitation, had occurred.

Mammals. An interesting experiment, representing a type of imitation, or at least social conditioning, was performed on 7 pairs of dogs by Kriazhev (**102**). With food or shock used as the primary stimulus, 1 dog of each pair was conditioned to a metronome or bell while the other was neither fed nor shocked, but only perceived the secondary stimuli given the first dog. When food was used, conditioned secretory responses, the stability of which lay intermediate between CR's of the first and second order, appeared in the observing animal. With shock, a rise in general excitability and conditioned respiratory change was shown by the observing dog, as was also the case in the food situation. Presence of another animal facilitated the formation of conditioned responses in the subject receiving the primary stimuli. No mention is made of dominance relation between subjects, so that results may not be compared with those of James (**100**), reported in the section on dominance, which were obtained under somewhat similar experimental conditions.

Primates. A unique methodology, which primarily emphasized social factors in imitation, was used by Aronowitsch and Chotin (**111**). The results of tests with it, however, were somewhat disappointing. From a group of 20 rhesus monkeys observed in a cage, the 2 most dominant and 5 others were selected, the most dominant being placed in cage A and the 6 others in cage B. The monkey in cage A was trained to open a food box when a door was raised exposing a red light, and to inhibit the response when a blue light appeared. For training of the 6 in cage B the signals were reversed, and 3 of them, who did not learn in the allotted time, were eliminated. Seventeen tests of imitation were made on 5 days by first testing the animals in each cage to insure presence of original training, and then removing an opaque screen between the cages and exposing signal lights in view of all animals. The results of many social test series were inconclusive because, on the preliminary tests, animals in each cage failed to react according to training. In one series of tests, of unreported number, 2 of the 3 followed the dominant monkey on half the trials, and the third on none of the trials, while the dominant was reported to have followed the group on 44% of the trials. On tests with the screen in place, subsequent to this series, animals in each cage reacted according to training. Tests should be repeated with this interesting methodology, using a greater number of animals of varied social characteristics.

The most convincing demonstration of imitative learning in the monkey or any other animal was given by Warden and Jackson (**144**), using a methodology designed to meet the criteria of intelligent imitation stated by Warden, Jenkins, and Warner (**175**, p. 256). These criteria are: "(a) the general pattern of the required response must be sufficiently complex and novel; (b) the response must appear consistently from mere observation, opportunity for practice being altogether excluded; (c) the performance of the imitator must be substantially the same as that of the imitatee; and (d) a sufficient number of instances under varied conditions must occur to eliminate the chance factor." Warden and Jackson used 2 adjacent cages, each of which contained identical, simple problem boxes. For a demonstration trial the box in the demonstrator's cage was illuminated by a spotlight and he opened it 5 times in full view of the imitator. This light was turned off in the demonstrator's compartment and another spotlight illuminated the imitator's problem box, which he was given 60 seconds to open.

Four problems were devised by using different fastenings on the problem-box door; 15 animals served as imitators, while a single monkey did all the demonstrating. On every problem each imitator was given a complete series of 6 groups of 5 demonstration trials, followed by 1 imitation test, regardless of his success or failure on the early imitation tests for each problem. For an animal who opened his box on the first imitation test, subsequent tests could be only tests of retention, yet the authors gave these equal weight with the first tests in reporting the general figure of 46.3% "immediate imitation." In the present writer's opinion this figure should be reduced to between 20% and 25%, depending on how certain doubtful cases are scored. Spence (**140**) has also criticized this study on the ground that the factor of enhancement of the stimulus was not entirely ruled out.

Crawford and Spence (**121**) recently have reported a new method for studying imitation which employed discrimination learning as problem material. A chimpanzee previously trained in discrimination learning was given a series of opportunities to observe another animal's discriminations between a pair of stimuli and then to make critical, irregularly rewarded choices for himself. The authors argued that this technique is superior to the problem-box method because learning is limited to a single connection between an already mastered manipulation habit and a visual stimulus, and because, by varying the combination of form stimuli, problems may be set which are known to be within the individual learning ability of the imitator, yet novel to him. Of the 3 experimental techniques used, which differed in procedures for rewarding the imitator on demonstration trials, only the one in which the imitator received no reward on demonstration trials can be considered a real test of imitation. Eleven test series made on 9 animals with this technique yielded unambiguous positive results in only 1 case and partial learning in others. The problem for the imitator was too difficult to make practicable a longer series of tests.

DOMINANCE

More attention has been given by experimenters to social dominance in vertebrates than to any other single topic treated in this review. Although the early writers were clearly aware of the phenomenon, particularly in birds (**48**, **92**), it remained for the Norwegian scientist, Schjelderup-Ebbe, to elucidate the principle of hierarchies, in his 1922 paper on domestic chickens. Since then, investigations have been made on every group of vertebrates save the fish and amphibia. Fighting and defense of territory have been described in the fish (**7**, **18**), and doubtless someone will soon attempt to work out a dominance hierarchy with members of this group.

As a fundamental criterion applicable to all vertebrates, dominance status is indicated by superiority in fighting ability of one individual over one or more species-mates. It is often impractical to observe all fighting which may take place within a group of animals under observation, so that in certain investigations relative dominance status has been inferred from other behavioral indices or indirect measures adapted to the peculiarities of the species. As more complex vertebrates are studied it is possible that more diverse manifestations of dominance status will be recognized in the individual. It is therefore important to discover how many behavior patterns of a single animal may indicate dominance, and to what extent they are correlated. A high correlation between a number of behavioral indices might indicate that dominance can be considered as an attitude which influences all the social behavior of the individual.

Exploration of the significance of the concept of dominance has hardly begun, since little is known as yet about how dominance relations may influence all sorts of social interaction. Also, the factors which determine dominance status are yet to be fully elucidated. Among the primates the subject is particularly fascinating, since there the relation between dominance and sex behavior is perhaps most striking. Full exploration of this relationship may suggest solutions for many baffling problems of sex attitudes and perversions as well as indirect means of social control.

Closely related to dominance is the phenomenon of territorialism, which has been described for representatives of most classes of vertebrates. A limited geographical area is settled upon and defended by an animal, usually a male, and is used by him for mating and rearing of a family. While a description of territorialism as observed in natural habitats is outside the scope of this paper, its importance must not be overlooked, since those factors which determine dominance status probably also influence the relative amount of territory acquired by an individual. In strongly territorial species little dominance fighting is observed, once territory is established, but when conflict does occur resident animals usually have a decided advantage over invaders. In testing for dominance status, with some species at least, psychological factors of familiarity with the environment may influence the results profoundly, since the "home cage effect" has already been clearly demonstrated. Territorialism in birds has been studied in some detail by field observers (**51**, **52**, **64**, **73**, **91**). Carpenter (**115**) found interesting territorial relationships between clans of howler monkeys observed in their natural habitat, while dominance behavior within the clan seemed to be lacking.

Reptiles. The comprehensive paper of Noble and Bradley (**28**) serves as an introduction to the work with lizards, since a summary of earlier studies on dominance, territorialism, sex recognition, and mating behavior is given. These authors observed 12 representative species in the laboratory and found that fighting occurred in all genera save *Amieva,* which is known to be gregarious in the wild, in contrast to the territorialism of individuals of other genera.

Typically, when 2 males meet, a display takes place in which each erects its dewlap or crest to its maximum extent, as he walks sideways toward his opponent, flattening his body in the vertical plane. Bobbing up and down rhythmically increases the visibility of the colored sides and under-surfaces of those males which possess such sex dimorphism. Often, bluffing is effective in driving away a rival, particularly in highly colored males, although when one male does not give way actual fighting with the teeth results. Fighting among males, which leads to the acquisition of territory in the wild, occurs during the breeding season.

Evans (**22**) employed a method specially adapted for lizards to work out a hierarchy among a group of *Anolis carolinensis.* Among a group of caged lizards only 1 animal appeared dominant, while all the others were subordinate and showed no differentiation among themselves. Evans placed 19 adult males in one cage, and, as soon as one had asserted dominance through fighting or bluffing all comers, turning the characteristic bright green color, he was caught, weighed, marked, and transferred to a second cage. As soon as another asserted dominance, he was transferred to cage 2. The first male to become dominant in cage 2 was moved on to cage 3, and so on until 6 cages had been used. As Evans seems to agree, the experimental determination of a hierarchy in lizards necessarily yields a result which, from the point of view of natural conditions, is an artifact. By arranging the animals in 4 groups, in accordance with the extent to which they maintained, in successive cages, the order of dominance assertion shown in the first cage, a fairly straight-line relationship was found between averages for these 4 groups and weight. Ninety of the 98 decisive combats occurring between a dominant animal, already resident in a cage more than 24 hours, and an invader were won by the resident. Yet in 38 of these 98 encounters the lighter animal won. Evans took these victories over heavier animals to indicate the marked effect of the "territorial urge" (p. 108). Kramer's study (**26**) of 4 male and 3 female green wall lizards *(Lacerta milisellensis)* confirmed many of the points of Evans' work.

Evans also explored the hormonal control of fighting in lizards (**20**, **21**, **23**, **24**). He injected a group of males with Antuitrin S and another with sheep pituitary extract, both of which resulted in fighting and courting by males outside the breeding period. In another experiment castration of females was found to liberate fighting behavior. This work is significant as indicative of a relation, even in the lizard, between sex and dominance behavior. In a Cuban field study (**25**) Evans explored the relation between behavior manifestations of dominance and territorialism. He was able to map the territories of 47 individual males and 8 individual females by placing a test specimen somewhere within the area and observing attack by the resident. Territories averaged some 400 square feet and usually contained an elevated "lookout post."

Birds. Schjelderup-Ebbe is the most prolific as well as the first writer on dominance in any animal. His first scientific report appeared in German in 1922, although unspecific reference is made (**88**, p. 952) to a paper published in Norway in 1913. The 1922 paper (**78**), which has been extensively reviewed in many places, was concerned entirely with the domestic chicken, while later papers (**79–87**) describe observations on upwards of 60 representative species of Carinatae, or keel-bearing birds, including many common European land and water forms and some tropical species. He says that, in general, the phenomena of dominance are the same in all the birds studied, the chief differences lying in the variations in intensity of fighting and consequent importance of ranking as a determinant of social interaction. The author gives much more attention to general descriptions of his findings than to accounts of observational methodology and behavioral details, so that it is often difficult to evaluate his general summary statements. Papers after 1922 are disappointingly repetitious, in so far as they concern dominance. An English summary of his work was prepared by the author (**88**), and other summaries have been given by Katz (**53**, **169**).

Schjelderup-Ebbe's original observations on *Gallus domesticus* employed large groups containing 25 to 150 individuals, and small ones ranging from 2 to 25 members. The pecking of one chick by another was used as the index of dominance status, observation of which revealed regular " peck-orders " among all groups, sometimes linear but more often polygonal. The " peck right " was found to be unilateral, *i.e.* in 1900 observed instances of pecking, if animal A once succeeded in worsting B, B thereafter was never observed to peck A, except on the rare occasion of a general revolt against the despot.

Dominance behavior appeared between the 20th and 30th day after hatching. Rank was roughly related to age and strength, and was maintained, once established, with little combat until senile changes during the 5th year altered the appearance of an animal, whereupon it was attacked as a stranger by other members of the group. The severity of dominance behavior was inversely related to rank; the despot over many pecked less often and less severely than one farther down the hierarchy. Removal of birds dominant over such an individual resulted in diminishing the severity of his treatment of his subordinates.

According to Schjelderup-Ebbe (**82**), factors influencing dominance include: (1) relative size, strength, and age; (2) circumstantial or attitudinal conditions attending the first meeting of 2 birds; (3) sex (in the relation between cock and hen, in which the cock is known to grant privilege during the mating season to his hen); (4) health; (5) familiarity with surroundings; and (6) the development of special friendships which cut across dominance lines.

To check Schjelderup-Ebbe's work under more precisely defined conditions, Masure and Allee studied the domestic chicken and pigeon (**31**, **57**) and also the shell parrakeet (**58**). Two groups of 11 male and 26 female (later reduced to 13) brown leghorn chickens were observed at feeding time in enclosed yards for 70 and 60 days, respectively, beginning in the 10th month after hatching. A linear dominance hierarchy appeared in the pullets before observations ended, but triangular relations persisted among the males. Much more fighting took place among males than among females. The chief result of some 30 days' observation of 7 male and 7 female white king pigeons, in separate groups, was the discovery of a "peck dominance" rather than a "peck right," as claimed for all birds by Schjelderup-Ebbe. In the former, no one of a pair of birds wins all the contacts, but only a majority, while in the latter the dominant animal is never pecked by the subordinate. When the sexes were paired, mating took place between most individuals and the whole group was dominated by 2 aggressive, unmated females.

The shell parrakeets, which live in flocks of some 1000 individuals each in their native Australia, were observed in 2 groups of 8 birds each, 4 of each sex, and 1 group of 14, equally divided as to sex. Again, no absolute peck right was discovered; a record of contacts showed that no one animal won all its contacts nor did any lose all. When the like-sexed flocks were mixed, all the females dominated all the males.

From their observations, Masure and Allee agreed with Schjelderup-Ebbe's description of fairly straight-line hierarchies and absolute peck right in *Gallus domesticus,* but failed to confirm his assignment of clear, linear dominance to pigeons or shell parrakeets, although these authors were not sure that hierarchies might not have appeared in these latter species with a longer period of observation. They also found that the top-ranking birds were the most aggressive; in one group of chickens half the pecking was done by the dominant animal—again in contrast to Schjelderup-Ebbe.

In an ambitious attempt to formulate social law, Murchison (**66**, **67**, **68**) set out to relate dominance as measured by direct observations of pecking in *Gallus domesticus* with 3 other tests of social behavior: (1) the relative distances 2 animals would move toward one another in a narrow runway; (2) the frequency of treading pullets by a cock; and (3) the differential approach of a chicken to 1 of 2 " stimulus animals " in a discrimination setup. Although as many as 18 subjects were used in part of this work, most of the results were obtained with 6 cocks and 5 pullets. The straight-line relationships obtained between various measures can be accepted only very tentatively, since they were based on 6 and often on only 4 or 3 points. The data, therefore, do not justify the extensive mathematical treatment (**70**) given them by the author. The results tended to show that the animal who won fights from the greatest number of others also moved further and faster in the runway toward any other animal in the group, and that the dominant cock did most of the treading of the pullets. In choosing between 2 males, a male usually approached the less dominant one, while a female would approach the more dominant of 2 cocks. Measurements were made with each test from the 1st through the 36th week.

In subsequent papers (**69**, **72**), the above behavioral measures were reported to be related to loss of body weight during 48-hour starvation, but not to any of the morphological characteristics determined by post-mortem examination. Using new groups of subjects, Murchison (**71**) found that the elapsed time before the formation of a linear hierarchy varied as a function of the size of the groups.

In connection with a study of the sex behavior of chickens, Skard (**90**) obtained a correlation of +0.54 between the frequency of treading by 1 cock of 10 hens and the peck order of the hens. In another group the correlation was +0.24. Miya's (**65**) report on observations of 3 groups of chickens substantiates the descriptions of dominance in these birds already presented. In addition, this author found an increase in the tendency of 2 strange chickens to fight when members of each contestant's group were present. Katz and Toll (**54**) found some relation between relative dominance status and performance in a simple learning test.

In their study of social behavior of the black-crowned night heron, Noble, Wurm, and Schmidt (**75**) used a unique index of dominance status. When a pair of birds performed the " billing ceremony," the one which held its head highest was considered dominant. This, they explained, is a habit perseverating from the time when young animals in the nest competed for food brought by the parents by stretching their necks upward. Strong territorial habits of even the immature birds of this species prevented the use of pecking or performance in Murchison's runway as criteria of dominance.

Glass cages, smaller than territories normally occupied by pairs of young birds, were arranged, and groups of 8, 7, and 7 subjects, drawn from 15 individuals, were placed 3 at a time in each cage, and the billing was observed from 2 to 10 days, or long enough to obtain a reliable result. From 0 to 147 billings occurred during a day. The results showed a linear hierarchy in all but one of the groups, where 1 triangle appeared. Birds which were tested in more than one group occupied the same relative positions with respect to each other in every group, although in one group they all ranked at the top and in another at the bottom of the hierarchy. The test behavior of pairs of birds who had lived together on territories was not distinguishable from that of those pairs which had experienced no mutual contact save in occasional defense of territory. Concerning territorialism and dominance, the authors stated that the peck order in herons "regulated the formation of pairs but seems to be of little practical value in the social life of captive herons once the territory drive has isolated pairs of young birds" (p. 22). These authors found, in supplementary experiments with pigeons, that birds accustomed to a cage invariably won fights against invaders.

Mammals. Most of the studies on mammals offer only fragmentary data. In a preliminary note Schjelderup-Ebbe (**105**) stated that he made observations on a variety of domestic and caged mammals in which dominance was observed in various degrees of clarity. Unfortunately, a full report of this work seems never to have appeared. Davis (**98**) presented a 7-point rating scale for measuring "aggressiveness" of a rat against its fellows (in contrast to aggressiveness against the observer's hand, etc.); the same measuring instrument might be used in a study of dominance. James (**100**) employed 5 dogs to study the effect of the presence of one dog upon the conditioning of another, and found that CR formation was inhibited in the subordinate dogs, although the results may have been due to direct interference by the dominant dog at the food delivery apparatus. James observed that dominance in the living cages was expressed by (1) fighting, (2) taking food from another dog, (3) usurping choice resting places in the kennel, and (4) driving away another dog in order to receive attention from a human being.

Winslow (**110**) described the type of dominance he saw among laboratory cats in a group of unreported size. Despotism of a single animal and undifferentiated subordination characterized the group relationship. The dominant animal mounted all others in a copulatory fashion regardless of their sex. When the despot was removed,

" communism reigned " for a few days and then the next most aggressive male emerged as despot.

By far the most satisfactory study on mammals, other than primates, was made by Uhrich on white mice (**106**). Some 300 males and 150 females, in a total of 85 groups, were observed in their living cages from 2 to 6 hours daily for continuous periods ranging from 3 to 268 days. Groups contained from 3 to 9 individuals. Fighting was used as the sole criterion of dominance, and data were recorded in terms of the predominant type of fighting observed on each day. The groups were classified into 4 somewhat overlapping categories for those showing (1) exclusive dominance, (2) a variety of types of fighting order, (3) linear dominance, and (4) no established order. Exclusive dominance of a group by a single animal, while the other members remained undifferentiated, was most frequently observed. Resistance to the dominant animal occurred occasionally and was considered a transitional stage, as were, also, cases of apparently equal dominance. A hierarchical arrangement of 2 or 3 animals was sometimes observed among the larger groups, but the linear order never extended further.

The temporal duration of a dominance order of any type was extremely variable, and changes took place gradually, abruptly, or with an interval of quiet in which no dominance was apparent. In most cases the inciting cause of change in order was not observed, while a few followed artificial types of interference with members of the group. Dominance status was found unrelated to weight, but some indication was obtained for a positive relation with age. When a new animal was placed in a group, the dominant animal usually attacked it. In living cages provided with a feeding device at which only 1 animal could eat at a time, no consistent order of feeding or striking inequalities of food obtained by dominant and subordinate mice could be detected. Evidence was obtained for a " home cage effect " in the augmentation of fighting ability of individuals against nonresidents or those removed from the company of familiar cagemates.

In addition to the above, 18 pairs of males were observed for an average of 53 days (range 13–105), during which complete dominance by 1 animal was found on 42% of the days, no fighting was noted on 48%, and the remaining 10% was classified as unknown or changeable order. In commenting upon all of his data Uhrich warned that caution must be exercised in generalizing on types of social behavior found when only a few groups of any species are studied. He concluded that the "social level of organization" is obviously much lower in mice than in many mammals and birds, and that mice do not always "know" who is dominant in their group.

Primates. The obvious relation between dominance and sex behavior adds to the complexity of the manifestations of dominance among the primates. The early observations of Hamilton (**126**) and of Kempf (**129**) on macaques and baboons suggested that sex behavior served functions other than reproduction. Strangely enough, Rêvész (**139**) failed to report observations of sex behavior when he tested for dominance in monkeys of various sorts. He vividly described the cowering of subordinate monkeys, as soon as a bit of fruit was held in front of the cage, regardless of whether the despot of the group threatened them at the moment or not. Incidentally, a peculiar place of privilege was granted the youngest and smallest monkey of the group; he was able to sit by the despot at the front of the cage and take food from the experimenter. Rêvész explained that the youngster had not yet begun to fight and hence had not learned to become frightened.

Zuckerman (**155**) obtained his original data from observations on baboon colonies in their native South Africa, on groups of some 100 individuals maintained on "monkey hills" in the London and Munich Zoological Parks, and on baboons and other primates caged in various groupings. He described how the dominance of the male overlords controlled all social interaction in the baboon groups, including the obtaining of food or any desirable object or attention, the handing down of punishment from more to less dominant animals, and the occasional rendering of assistance to a subordinate against an attacker. In a very large proportion of the cases dominance was clearly manifested by sexual behavior; the subordinate animal characteristically presented to the aggressor of either sex. Zuckerman argued that since "both the male and the female primate are always to some degree in a sexually receptive condition" (p. 238), and since various reproductively inadequate stimuli are effective in evoking sexual behavior, sexual responses must have become conditioned to fear-producing stimuli, and presentation may therefore be regarded as an incipient flight response. This hypothesis must be examined for each type of primate in the light of what is known about periodicity of receptivity in the female. Its application to the male implies some sort of imitation of the females by the males, a type of primate behavior which is popularly regarded as very prevalent, but actually difficult to demonstrate in any large portion of experimental cases.

Maslow made a series of observations on a variety of monkeys from which he was able to describe a "behavior syndrome of dominance." The first paper (**133**) reports observations on 35 Old- and New-World monkeys and baboons which were caged in groups of 2, 3, and 4 animals. Qualitative records were kept of significant behavior appearing when food was introduced into the cages. In a second paper with Flanzbaum (**136**), a systematic test method was employed with 23 monkeys (mostly macaques), which were kept in isolation save when they were paired for daily test sessions. This same methodology was subsequently employed (**135**) with 6 rhesus monkeys, which were tested in every possible pairing. Food was placed in the reaction chamber at regular intervals during each test period, and records were made on check lists of amount of food obtained by each subject and the frequency of various types of behavior displayed by each.

Results from both these experiments showed that in general the animal which obtained the most food also did most of the sexual mounting and bullying (plucking fur, biting, threatening with growls, mauling, etc.), and initiated most of the fighting, while it was never observed to cringe, was rarely passive under aggression, and almost never took flight from its partner. Frequency of grooming, sexual presentation, and play behavior did not consistently differentiate members of the pairs. The numerical scores presented for these items are mutually incomparable because they were calculated in different manners and were based on varying numbers of cases. Food division was the only uniform test applied to all animals, the other observed behavior occurring spontaneously and irregularly. The difficulty of quantitative description encountered here is typical of that met whenever the attempt is made to subject personality traits to measurement.

Maslow found that the behavior of most pairs in the test situation became consistent before the 4th period, often before any fighting occurred, which suggested to the author that relative dominance status may be represented by attitudes, gestures, or other nonscorable behavior, which another animal may recognize at once and react to accordingly. On the other hand, the lack of perfect correlation between obtained scores led the author to suggest that there may be specific types of dominance (food-, sex-, and aggression-dominance, etc.) rather than a general dominance. In comparing this work with that reviewed for other vertebrates it should be noted that Maslow did not use the outcome of fighting as the primary index of dominance.

After a hierarchy had been worked out with the results of all pairings of the 6 animals employed in the last study (**135**), a larger group was formed by placing the animals together in inverse hierarchical order. It was found that concerted (not necessarily coöperative) attacks on higher ranking animals by subordinates upset the calculated hierarchy. Finally, Maslow offered a theory of dominance (**134**) in which he attempted to account for most nonreproductive sex behavior as a function of a dominance drive; but at the same time he appears to have regarded this as sex behavior differing in kind from that employed for reproduction, thus becoming involved in the logical difficulty of trying to prove, from the same original data, the existence of both a new type of behavior and a new drive.

FAMILY LIFE

Within the family group development of the social responsiveness of young animals, as well as patterns of maternal care and control of infants, may be observed. While naturalistic observations have been made in this area on a variety of vertebrates, the field has been left almost untouched by experimental methodology. Most of the controlled laboratory observations have been made with chief interest in the unfolding of innate behavior patterns in the mother rather than in the genesis of social responsiveness in the infants (**109**).

Fish. Two observational and experimental studies have been made on family groups by German authors, whose interest lay in tracing the transition from what Deegener (**164**) called the *Gynopaedium* and *Pateropaedium,* or mother- and father-families, to the *Sympaedium,* or aggregation of independent young. Schulz (**12**) observed in the laboratory some 50 broods of sticklebacks, one of the unique types of fish in which the father guards the nest and cares for the young. Tests were made which showed that the father's behavior was not greatly altered by the introduction of young from other nests or by reduction of the number of young in his own nest. Of particular interest is his observation that when the young became mature enough to emerge from the nest the father swam about with them as a member of the school, but not in any sense its leader.

Birds. Brückner's extensive study (**37**) on the breakup of mother-families in domestic chickens was made on 36 groups of hens with chicks or incubator-hatched chicks. Three stages in the life of the mother-family were distinguished: (1) concentration phase, lasting from hatching until the 2nd or 3rd day and characterized by very close association between hen and chicks; (2) fluctuating contact phase, lasting until the 10th or 12th day and characterized by short excursions of chicks from the hen, and clucking apparently directed toward specific, straying chicks; and (3) the dispersion stage, lasting from the 12th day through the 6th or 8th week and characterized by a much greater freedom of the chicks but continued response to the hen's clucking. Breakup of the families took place rather quickly. The hen ceased clucking, pecked at the chicks when they approached her, and finally began to lay again and would no longer tolerate members of her brood.

Brückner concluded from experiments in which the visual appearances of hens were altered that chicks recognized their mothers by both visual and auditory stimuli. Chicks of 2 families were mixed in the dark and were found grouped under their own hens within 2 hours. The author found evidence that hens recognized chicks as belonging to their own brood, but not as individuals. Among chicks, individual recognition was first evident when fighting and the process of rank-ordering began at about the 14th day and continued without serious interference from the hen. On breaking up of the family the chicks remained in a group, or *Sympaedium,* for some time, even though immersed in a larger group of adults in the henyard. The hen, on leaving her chicks, joined the adult group if such was present; otherwise, she remained with her chicks and defended her position in the peck-order as long as she was able. Studies of the adoption of strange chicks into the mother-group were made in which success seemed to depend on similarity in appearance and behavior of the newcomer to the brood. A type of coöperative behavior was sometimes shown by 2 hens who reared their broods together. Brückner found no evidence for tuitional behavior by the hen.

Primates. Of the naturalistic observations, those made on primate mother-infant groups may be singled out (**113**, **118**, **131**, **142**, **149**), since the relatively long period of infant dependence and the strong social responsiveness of these vertebrates will doubtless provoke systematic and experimental study in the near future. Yerkes and Tomilin (**152**) amassed considerable data on 5 mother-infant groups of chimpanzees and found as many varieties of maternal treatment and infant response. Some description of the early development of social behavior between chimpanzee twins was given by the same authors (**143**).

EFFECT OF ISOLATION ON SOCIAL BEHAVIOR

By the early isolation of young animals the effect of deprivation of parental care and companionship on subsequent social behavior may be studied.

Birds. Craig (42) began this work by rearing 3 male doves in isolation for periods of from 1 to 3 years and studying their initial responses to females. Two of the 3 made sexual responses to the experimenter's hand which fed them; all showed maladaptation to mating behavior when paired with a female, but after some practice they became proficient. The symbolic function of the experimenter's hand as a stimulus for all sorts of social and sexual behavior is perhaps the most interesting result of this study.

Brückner (37) reared 2 domestic chicks in small wooden boxes in a brooder. The chicks could see shadows of other chicks through translucent windows of the boxes and could hear them. When alone, they developed various types of play and tossed worms and other foods about as if other chicks were present and competing for them. On pairing each isolated chick with another chick at 7 weeks fighting occurred, as well as pecking at a mirror image of itself. On being released into a group of 250 chicks the isolated chicks showed bewilderment and retreated to secluded nesting places as soon as possible, to avoid pecking by the others.

Two experimental studies have recently appeared in which the effect of early isolation on social responsiveness was subjected to measurement. Pattie (76) arranged an experimental pen with 2 stimulus compartments, one containing 2 chicks and the other, 2 white mice. Beginning with the 4th day of life, 42 isolated chicks and 42 normals were placed one at a time, daily, for 30-minute periods, in the test pen, and the time spent in front of each stimulus compartment was automatically recorded. Social preferences measured in this way over the whole 6 days showed no significant difference between isolated and controls, but, when the first 3 days were considered, a preference of the isolated chicks for other chicks was shown, which waned in favor of a preference for mice on the last 3 days.

Mammals. Bayroff (94) separated the members of 6 litters of rats from their mothers at 19 days and reared half in isolation and half together. Subjects were selected for experimental and control groups by being paired for activity, one being placed in isolation and the other in the social group, 47 subjects being used in all. After a preliminary training period, begun on the 115th day, all animals were given 60 daily opportunities to choose between compartments containing 2 other rats and food, and only food. A second experimental series employed only stimulus animals *vs.* an empty compartment. No consistent differences between the social preferences of rats reared in isolation or in groups were found. Perhaps the rather disappointing results of such careful experimental work on this topic come from an overeagerness for quantification to the neglect of systematic qualitative observation which might indicate striking peculiarities in interactive behavior of socially naïve subjects.

Primates. Infant primates have been reared in isolation for the chief purpose of observing maturation of innate behavior patterns and study of infant mentality under controlled conditions. Jacobsen, Jacobsen, and Yoshioka (**128**) reared a chimpanzee from birth to 9 months, and described the first responses of the infant to another infant at the end of isolation. Aggression by the young ape was replaced by dependence on her own companion after a few weeks. Foley (**124, 125**) reared a male rhesus monkey in isolation for 2 years and reported on responses to a mirror and to other animals occasionally presented during the isolation period. No detailed account of the initial adaptation to life with other monkeys after the isolation period was given, beyond a general statement about genital exploration and preference for females by the experimental male.

COMMUNICATION

The term *communication* lacks precise definition in its application to animal behavior, for it may be stretched to include almost any sort of anticipatory movement which may signalize an activity to another individual, or it may be limited strictly to vocalizations or gestures which clearly direct or predicate. While communicating in some sense is obvious in at least many birds and mammals, the possession of a " true " language by any animal has long been a subject of controversy. Bierens de Haan has written extensively (**35, 161, 162, 163**) on this question, reviewing the literature on animal communication through 1934. He argues that animals do not possess a human type of language, because no reported instance of animal communication has shown all the characteristics which he believes distinguish the elements of human language (vocal, articulate, indicative, conventionalized, intentional, and capable of regrouping to give new meaning). This author finds that some of these characteristics are displayed in a few instances by some animals, but they are never all present at once. Groos (**168**) presents arguments of the same nature.

While there have been numerous descriptions of the calls of animals made in their natural habitat, the objective recording of these and the careful description of the conditions under which they occur have not often been made (**33**). Perhaps the available information (**40**, **49**, **50**) may best be summarized by saying that the calls, especially of birds, have in some cases been found to fit into regular patterns of recurrent behavior and probably serve as signals to release, in Lorenz's (**55**, **56**) terminology *(vide infra)*, or to inhibit the reciprocal behavior on the part of other individuals. The more or less stereotyped behavior immediately preceding flight, copulation, etc. has been regarded as an effective means of communication among birds (**43**).

The natural modes of primate communication and the attempt to elicit vocal response for problem solution have been adequately treated by Yerkes and his associates in various places (**151**, **153**, **154**). Taking the suggestion of Bloomfield, amplified by Esper (**166**), that the "fundamental linguistic situation" is that of coöperative action involving 2 or more individuals, Crawford (**119**, **120**) observed gestures used by chimpanzees in coöperative problem-solving, and found that, in some instances, they showed an element of directiveness, or predication.

Related to the subject of communication in animals are studies of sound mimicry by birds (**39**, **77**, **89**) and the native or acquired nature of call and song. Schjelderup-Ebbe's work (**81**) remains the only extensive study in this field where large enough groups are employed for statistical reliability. After carefully describing 13 types of vocalization in chickens he experimented with 360 incubator-hatched chicks in 6 groups. The groups were reared in varying degrees of visual and auditory isolation from adults. It was found that crowing by young males was the only vocalization which appeared earlier in chicks within sight and sound of adults than in isolated groups. The most objective work in this field is being carried out by Metfessel and his co-workers on the canary (**30**, **38**, **59**, **60**, **61**, **62**, **63**).

Also related to the topic of communication are the attempts to teach animals to use human words (**101**; other studies reviewed in **153**, **161**). The most recent work on the reactions of vertebrates (dogs) to verbal commands was done by Warden and Warner (**107**) and Sarris (**104**). The former's critical examination of the famous dog, "Fellow," revealed considerable ability of the animal to follow commands. Sarris' extensive work on 3 dogs, in which the ability of each animal to use verbal cues in diverse types of problem solution was studied, suffered from lack of complete control of possible visual cues and often from an inadequate number of trials in various problem situations.

SPECIAL CATEGORIES OF SOCIAL RESPONSE

Response of the Group to an Individual. A few investigations on birds and primates have been found which offer suggestive beginnings toward definition, description, and analysis of the concept of a social group. Work in this highly significant area should be pushed ahead with animal subjects, where composition of groups and stimulus conditions are subject to experimental control.

(1) *Birds.* Fischel (**45**) observed 500 domestic hens in a large open yard and found that they almost always moved and fed in groups. Direction of movement of the groups seemed to be defined by the features of the terrain and the wanderings of "leaders" who moved at the apex of each group. A number of animals seemed to race for this position of *Spitzentiere,* and, while the same individual did not always gain the place, it was usually held by one of a limited number of hens. Dependence of this leader on the group was indicated in the way she would wait for others to catch up if she moved more than a little distance away from them. Leadership and position in the dominance hierarchy did not seem to be related. It is doubtful whether *Spitzentieren* may be called real leaders, since one hen was not always leader, nor was it observed that her example was followed in anything other than locomotion.

A very suggestive type of work was begun by Schjelderup-Ebbe (**81**) on the recognition by group members of individuals belonging to the group. He altered the appearance of hen's heads by covering the comb with a bonnet, or by coloring comb and head feathers with various dyes. In most cases the transformed member of the group was treated as a stranger and was forced to work her way into the closed dominance hierarchy through fighting. Alteration of appearance through sickness and old age likewise resulted in aggressive treatment from the group. Suggestive techniques for this type of study were employed by Noble and Vogt (**74**) in their experimentation on sex-recognition in the red-winged blackbird and the northern yellowthroat. They used stuffed birds mounted in various positions and altered in different ways. Noble, Wurm, and Schmidt (**75**) studied individual recognition of 6 young black-crowned night herons when the appearance of individuals was altered and also when auditory sensitivity was eliminated. Observations on the recognition of offspring by parents should also be mentioned here [Heinroth (**48**), Whitman (**92**), Schjelderup-Ebbe (**81**, **83**), and Brückner (**37**)].

A very interesting theory bearing on the topic of group relationships has been offered by Lorenz (**55**, **56**). It is well known that birds of different groups or species make immediate, appropriate, and stereotyped responses to particular kinds of objects or individuals, especially those belonging to the group or species. Lorenz postulates that certain characteristics of coloration or plumage prominently displayed by birds during certain behavior serve as specific " releasers " for the stereotyped recognition behavior. This coördination of movement and response between birds may serve as a " private signaling code " for a group or species. Preparatory activities and feather displays made by birds before taking flight are the clearest examples of releasers. The connection between releasers as stimuli and the appropriate response is made early in life through the process of "imprinting," which Lorenz hesitates to classify as learning because it takes place during infancy, long before the response itself appears. Because of the completeness and irreversibility of the imprinting, Lorenz suggests an analogy to the " induction " of growth of transplanted tissue described in experimental embryology (Speman). Imprinting at once fixes the organism within the social group of whatever parents may rear it. The social relationships that will exist in later life will be strictly limited to the behavior which is thus released. For this restricted type of companionship, which may exist between individuals, Lorenz borrows Von Uexküll's term of *Kumpan,* which is not strictly translatable into English, but refers to a companion for a limited activity, as a hunting-companion or drinking-companion. Among birds, Lorenz discusses the characteristics of releasing functions for parent-companion, child-companion, sex-companion, social-companion, and brother-and-sister-companion.

(2) *Primates.* Using 5 baboons as subjects, Dolin and Palatnik (**122**) studied the response of a group to an individual's behavior in a learning experiment. The animals first learned to approach a food container at the sound of a bell, which required from 21 to 300 trials. Next, a lever was made available which could be operated by an animal to ring the bell. The conditioning of members

of the group to the approaching and manipulation of the lever by a single animal was followed, and it was found that individuals learned to respond to their fellows' movements in from 2 to 231 trials. The authors seemed more interested in the learning aspects of the problem than the social relations involved. Of course, the apparent response to the individual may have been simply an anticipatory response to the bell.

The possibility of the formation of a "taboo" by a group of rhesus monkeys against a single individual who was the instrument of their punishment has been reported in abstract by Child (**117**). Each animal was free to mount a platform to obtain a piece of banana, which resulted in the floor of the cage being momentarily lowered a half-inch into hot water, so that the other animals present in the cage were punished. Another platform was available from which a piece of carrot could be reached without resulting in punishment for the group. "Inhibition of the tendency to take banana appeared rapidly and persisted in each animal, the principal source apparently being undirected commotion of the other animals" (p. 705). While this attempt to build up a "taboo" in monkeys proved inconclusive, the methodology is extremely suggestive. If it could be shown that animals can exert a definite pressure against their fellows because of antisocial behavior, a way would be opened up for the experimental study of many baffling human problems.

Grooming Among Primates. Students of primate behavior have unanimously recognized grooming of one animal by another as an important pattern of social interaction. The term has been used to designate visual examination and picking through another animal's hair, in which objects discovered are usually transferred to the mouth of the groomer. Watson (**145**) early described it in the monkey, and Köhler (**130**, pp. 320–323) gave an accurate account of its typical form in chimpanzees. Zuckerman (**155**) made frequent reference to grooming among baboons, particularly that done on the person of the overlord by females in his harem who happened to be in the receptive phase of their sex cycles. Zuckerman maintained that the grooming response is evolved from a fundamental attraction which fur of any kind has for monkeys and baboons.

Tinklepaugh (**141**) concluded from observations on a pair of well-acquainted macaques that the male attempted not only to keep the female's hair clean, but also to alter her appearance to suit his taste by hair-plucking. Concerning the innateness of the grooming response, the Jacobsens and Yoshioka (**128**) reported its sudden are displayed in a few instances by some animals, but they are never all present at once. Groos (**168**) presents arguments of the same nature.

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Yerkes (**147**) summarized the evidence for the importance of grooming as a means of social interaction, and maintained that from it may "have evolved varied and highly significant kinds of social service" (p. 12). He recognized a functional identity between grooming in infrahuman primates and "delousing" in man. From his own observations Yerkes stated that grooming does not appear in chimpanzees until after the 3rd year, that it seems to be its own reward, apparently indicates friendliness, and is essentially a mutual activity which may represent a purely altruistic form of social service. As pointed out by this authority, much careful genetic and experimental work should be done to discover the importance of this behavior.

Food-Sharing and Coöperation in Chimpanzees. With the chimpanzee, a beginning has been made on the study of social interaction patterns resulting in the combination of resources or mutuality of effort. Köhler (**130**) observed food-sharing between 2 chimpanzees and vividly described the begging done by the ape desiring food (pp. 312–313). Begging other apes or human beings for all sorts of things or attentions has been noted by observers of orangoutang, chimpanzee, and gorilla (**132**, **153**).

Following observations of Wolfe (**146**) on the sharing of food-tokens by pairs of chimpanzees, Nissen and Crawford (**138**) arranged a situation for the study of food-sharing between 6 immature chimpanzees in 8 different pairings. Members of pairs were placed in adjacent cages connected by a grille partition through which an animal might reach, and food was given only to an animal in one cage. The other chimpanzee obtained some of it by reaching directly for it, by begging the partner for it, or through what appeared to be unsolicited passing by the partner having access to the food. The successful begging and unsolicited passing were found only with well-acquainted and friendly pairs; certain animals, however, begged vigorously in any pairing. The begging was done with arm outstretched, palm up, wrist moving up and down, and occasionally was accompanied by whimpering and foot-stamping. Discovery of motivation for food-sharing and conditions of its elicitation will not be revealed until chimpanzees of all ages are studied.

Opportunities for coöperation or teamwork by a pair of chimpanzees were offered in the tests on box stacking of Köhler (**130**, pp. 166–172) and Bingham (**114**, pp. 76–77), but mutual obstruction and independent activity characterized the behavior during the limited periods of observation. Carpenter (**116**) reported that he saw 2 gorillas, under observation in a zoological park, coöperate in moving a log, although he gave no details of the behavior. Crawford (**119**) employed various problem situations to study coöperation in animals formerly trained to do a similar task alone. In the situation yielding the clearest results, 2 animals were required to pull in with ropes a box too heavy for a single ape to move, in order to obtain pieces of fruit. Active tuition and assistance by the experimenter resulted in the first stage of coöperative behavior characterized by independent pulling on a rope in response to verbal command. Two other stages developed spontaneously. The second was characterized by watching the partner and response to its pulling; the third was distinguished by gestural solicitation of the partner's pulling. Of the 5 subjects employed all reached stage 1, in certain combinations all showed behavior typical of stage 2, and 2 solicited in the manner characterizing stage 3. It was argued that the solicitation here displayed had only a signalizing, and not a directive, significance for the partner.

Another experimental situation was arranged (**120**) which required pairs of animals to operate 4 devices in serial order to obtain food from a vender. After 3 subjects were trained alone in the serial habit (which required responses to color stimuli independent of location) they were paired with other apes who had learned how to manipulate individual devices but were unfamiliar with their serial order. During delays the trained animal was observed making gestures toward the partner, which resembled the solicitation previously observed, but which had an added element of directiveness, since the solicitor seemed to push, beckon, or draw its partner toward a particular one of 2 devices. The solicitation of the dominant animal, here as elsewhere, seemed to be of a commanding type, while that of the subordinate connoted suppliance.

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SOME PSYCHOPHYSIOLOGICAL RELATIONS[1]

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The present review is an attempt to summarize the experimental literature dealing with the changes in mental performance and function which follow alterations in the internal environment. The well-known phrase of Claude Bernard, "The constancy of the internal milieu is the condition of the free life," may well serve as a guiding text. It is apparent that the relationship between mental and physiological functions may be considered in either of two directions: first, how may physiological functions be altered by mental processes; and second, in what way will alterations in the physiological environment affect mental performance? In the present review we shall be concerned only with the latter relationships.

CHANGES OF OXYGEN TENSION

The effects of lowered availability of oxygen may be considered under immediate short-term effects or under the effects of long-continued oxygen lack in which a period of acclimatization may occur.

Short-Term Immediate Effects

The problem of the immediate effects of low oxygen content of the inspired air has important practical applications in the field of aviation, and it is in connection with the selection of aviators that the first experimental work was carried out. These early experiments have been summarized in detail by McFarland (**69**). However, the experimental conditions in these early experiments were such that little impairment in mental function was found prior to physiological collapse. In these experiments the anoxemia was built up over a relatively short interval of time by rebreathing expired air from which the carbon dioxide was removed, so that conditions of physiological equilibrium were not attained. Furthermore, the subjects were highly motivated, and the tests were directed primarily toward specific psychological functions in which practice effects could be minimized. McFarland found that choice reactions were impaired when the oxygen in inspired air fell to about 11% and gradually became worse with increased deprivation. After exposure of an hour or more, the effect of lowered oxygen was greatly accentuated. Simple sensory and motor responses were not seriously impaired until the subject approached collapse from oxygen want. Touch and vision were apparently the first sense modalities to be adversely affected and audition, the last. In these experiments the higher mental processes, with lack of memory and attention, were impaired first at relatively low oxygen deficits, although awareness of what was going on persisted until collapse. Irrational or fixed ideas were frequent, and capacity for sane judgment and self-criticism was lost. As in other experiments (5, 6), the subject was reported to be frequently unaware of the profoundly altered behavior and often insisted on continuing the experiment in spite of obvious dangers. Barach (3) is of the opinion that, after even slight oxygen deprivation, grave errors of judgment may be made and attributes certain aeronautical accidents to this cause. Perseveration, both with respect to ideas and motor responses, was frequently reported.

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Goralewski (42) studied the effects of anoxia, using a rebreathing apparatus which removed the carbon dioxide from the expired air but with a gradually decreasing oxygen supply. He used handwriting which had previously been found to suffer marked impairment under lowered oxygen tension (69). By making a rough comparison of handwriting specimens obtained under normal and anoxic conditions, the author stated that, in a group of 100 subjects, approximately 44% showed definite disturbances when the oxygen content of inspired air was between 14 and 18%; 22% of this group showed the first disturbances when the oxygen content was above 18%, and the rest, at concentrations below 14%. The maximum functional disturbances appeared in 52% of the subjects when the oxygen content was between 10 and 14%. Tanaka (118) used a low-pressure chamber to reduce the oxygen tension in a group of 6 subjects. After 7 to 10 days' practice on the part of the subjects in card sorting, controlled association and memory tests, the experimental series of 4 tests was carried out. In all subjects the maximum impairment was with addition, which occurred very suddenly when the oxygen content fell to about 11½%, corresponding to an altitude of 15,000 feet. At 21,000 feet, or about 9½% oxygen, the time required for simple addition increased, on the average, by 112%. With the same degree of oxygen tension, choice reaction time was increased by about 40%, while memory was significantly impaired.

Some evidence that the effects of anoxemia may persist is found in the following experiments by Tanaka, who administered the mental tests to his subjects under conditions of increasing oxygen tension after anoxemia; that is, after the oxygen concentration in the experimental chamber had fallen to 9%, or 21,000 feet, oxygen was admitted to raise the concentration to about 10%, equivalent to an elevation of 18,000 feet, at which time the mental tests were again administered. Tests were administered at 3000-foot intervals throughout the entire descending period. In general, the mental performances of both the ascending and descending period resemble each other, although the performance in the descending period, after oxygen deprivation, is uniformly inferior to performance at the same altitude or same oxygen concentration during the ascending period. The cause of this difference may be, in part, the fatigue effect of the experimental procedure or it may be an indication that the effects of low oxygen tension persist for a longer period than has been commonly supposed. McFarland (70, 73) has shown that the rate of change in oxygen tension is also important. Rapid changes produce more profound effects than slower changes of an equal amount.

One of the chief difficulties in investigating the effects of changes in physiological functions on mental performance is the effect of practice. If the experimental subjects are put through a training period, it is true that the effects of practice are minimized. However, if practice is continued so that there is no obvious improvement, it is evident that the task has lost much of its usefulness as a measure of excellence of mental performance. In other words, the activity has become firmly established, and the amount of cortical activity represented becomes minimal. Under these circumstances it is questionable whether results from such a test can be taken as an adequate index of mental capacities. Few experimenters have utilized the learning function in their procedures, although an approach to this technique was made by Barach, McFarland, and Seitz (4), who found that 3 out of 4 subjects showed a decrease in the number of problems solved on a slide rule and an increase in errors while working for 4 hours in a chamber containing oxygen percentages corresponding approximately to 12,000 feet. In these experiments the detrimental effect of low oxygen was observed in opposition to improvement from learning. McFarland, Knehr, and Berens (76) administered 12½% oxygen and 10½% oxygen to a group of 10 subjects and found an increase in the average time for reading 6 lines of print, as well as an increase in the number of fixations of the eye. A definite decrease in the precision of ocular movement was apparent at 12½% oxygen, corresponding to 13,500 feet, and was quite marked at 10½% oxygen, or 18,000 feet.

Lennox, Gibbs, and Gibbs (**67**) have shown experimentally that there is a sharp decrease in blood flow from the head immediately preceding and accompanying the loss of consciousness. These experiments were carried out in patients in which the record of relative flow of blood through the jugular vein was obtained. Blood analysis shows that unconsciousness results if the oxygen supply to the brain is suddenly reduced to such an extent that the oxygen saturation of the blood in the internal jugular vein falls to 24% or below. In this experiment a direct association is established between degree of oxygenation of the brain and mental function.

Gellhorn and his associates have also studied the effects of changes in the internal environment on mental functions. It was found that auditory and visual intensity discriminations were greatly decreased under the influence of oxygen deficiency, excess of carbon dioxide, or involuntary hyperventilation (**31**, **37**). The latent time of visual after-images was increased by the same physiological changes (**38**, **40**). Various processes, such as association (**36**), cancellation of numbers (**35**), and addition of digits (**35**), were all adversely affected by lowered oxygen tension. The influence of anoxemia on handwriting and memory was striking. Under the influence of anoxemia the handwriting tends to change in character: the letters become larger, more clumsy, lose their individual characteristics, and misspelling occurs. Finally, a state of complete illegibility is reached (**33**, **34**). Similar changes in handwriting are reported by McFarland (**69**) and by Barcroft (**5**) in cases of severe carbon monoxide poisoning.

Gellhorn and his associates found that all the sensory and mental impairments which result from oxygen lack were completely alleviated when subjects breathed from 3 to 4% carbon dioxide, even though the same degree of oxygen lack was maintained (**32**, **33**, **34**). This is true both of sensory perceptions and of higher mental functions (**33**), as well as handwriting (**33**). These experiments show conclusively that a small concentration of carbon dioxide in the inhaled air, which in itself is without any effect on the various physiologic functions and psychic processes investigated, either prevents completely or diminishes greatly the severe effects of anoxemia.

This beneficial effect of carbon dioxide has been found to be due to (1) its stimulatory effect on respiration, whereby the alveolar oxygen pressure is raised and thus a high degree of oxygenation of the blood is obtained; (2) the stimulating effects of carbon dioxide and oxygen deficiency on the vasomotor centers, resulting in vasodilation in the central nervous system, with a resulting increase in blood flow, which thus increases the effective oxygenation of the central nervous system; (3) the effects of carbon dioxide and oxygen deficiency on temperature regulation. These experiments are of far-reaching importance, although numerous physiological aspects of the problem are yet to be solved.

Previously, Schneider, Truesdell, and Clarke (**100**) had found no modification of the influence of anoxemia even when 4 to 8% carbon dioxide was breathed. These negative results may be attributed to the fact that 4 trained subjects, who were accustomed to work in a low-pressure chamber, were used in these studies. Examination of the data shows that a definite increase in respiratory volume was produced by the addition of 4% carbon dioxide. Furthermore, their data show that up to altitudes of 20,000 feet, the alveolar oxygen tension was greater in experiments where carbon dioxide was added to the inspired air in the presence of anoxemia. However, no subjective improvement was reported by the subject, and fainting and a general demoralization of the nervous system occurred in approximately the same atmospheric pressure with carbon dioxide as with the unmodified anoxemia.

Ruhl and Spiess (**98**) found that with carbon dioxide inhalation in anoxemia there is a considerable rise in arterial oxygen saturation, and symptoms of oxygen deficiency both in physiological and psychological tests are reduced.

Kraines (**65**) administered the Army Alpha intelligence test to 30 persons while they were breathing a mixture containing 10% oxygen, corresponding to an altitude of about 20,000 feet, and found a drop in performance of from 1 to 54 points. Subjective estimates of failing ability under low oxygen were found quite inadequate. Since the failing point of performance correlated with the appearance of instability of blood pressure, the author suggested that the efficiency of the cerebral cortex under the stress of anoxemia is dependent upon the ability of the circulatory system to compensate for the diminished oxygen supply.

Similarity between psychological symptoms of oxygen want and certain symptoms of dementia praecox led Hinsie, Barach, Harris, Brand, and McFarland (52) to subject a series of dementia praecox patients to oxygen concentration of about 50% over a period of 2½ months. Oxygen administration was given daily for varying periods of time. None of the patients showed clinical improvement; however, treatment with carbon dioxide was effective in producing remission of symptoms.

Long-Term Effects of Prolonged Anoxemia

It is well established that persons subjected to diminished oxygen supply over long periods of time will make physiological readjustments which will eventually result in a maintenance of higher oxygen tension in the tissues than that in subjects immediately exposed to the same oxygen concentration in inspired air. This process of acclimatization with a resultant increase in the number of red blood cells circulating in the blood, as well as changes in the vital capacity and respiration, may attain a measurable magnitude even after 12 hours of exposure to lowered oxygen tension. A period of weeks usually elapses before a final equilibrium is attained (71). Numerous studies of the physiological reactions of individuals who have spent most of their lives at a high altitude indicate that acclimatization is never complete—that is, that these individuals never attain as high an oxygen tension in their tissues as do individuals who live at lower altitudes. There is further evidence that the actual tissues of persons living in high altitudes make no adaptations and function at a consistently lower oxygen tension than those of persons living at sea level. However, very few studies of mental reactions and capacities of highland groups have been made.

McFarland (72) compared 35 miners, living at an altitude of 17,500 feet and working in a mine at an altitude of 19,000 feet, with a group of 35 Antofagasta workmen, who were of the same racial stock and average age as the group of miners and who were engaged in heavy manual labor at approximately sea level. It was found that the miners at high altitudes were slower and more variable in simple and choice reaction times than the workmen. Thus, significant differences in neuromuscular coördination were found. Auditory thresholds were higher for the group of miners than for the workers at sea level. This difference, however, as pointed out by McFarland, is quite possibly due to the peripheral effects on the eardrum, with regard to pressure, temperature, and wind, to which the group of miners was exposed. Before any judgment as to the prolonged effect of anoxia on mental performance can be made, experiments in which a wide range of intellectual capacities are tested in groups which are similar in all respects except altitude at which they live need to be carried out.

It has recently become apparent that prolonged anoxemia of the brain may result in permanent damage to the central nervous system, with attendant mental and motor dysfunction. Clinical reports by Hunt (**61**) and others (**101**) call attention to the frequent association of mental symptoms with pernicious anemia. Since no quantitative measurements of mental performance were reported, it is impossible to evaluate the importance of such clinical observations.

Schreiber (**102**) and Schreiber and Gates (**103**) reported instances of cerebral injury in the newborn due to anoxia at birth. They reported a high incidence of birth anoxia in infants where drugs such as scopolamine or morphine have been administered to the mother during delivery. Eastman (**26**) attributed numerous cases of asphyxia in the newborn to anesthesia of the mother. On the other hand, Ford (**30**) was unable to find any evidence of cerebral lesions in kittens after experimental production of anoxemia at birth. Carbon monoxide administration was also without effect, although the periods of administration were short. Studies of relationship between intelligence or mental development in children and the presence and the use of narcotics at birth have not been made, although such results would be of extreme interest and importance.

Courville (**21**) has collected a series of cases in which severe mental and motor symptoms have developed as a result of asphyxia induced with nitrous oxide anesthesia. Severe degeneration of the brain following nitrous oxide-oxygen anesthesia is reported by Lowenberg and Zbinden (**68**) and by O'Brien and Steegmann (**86**). The augmentation is attributed to anoxemia of the brain. Reports of mental involvement resulting from carbon monoxide poisoning have also been reported (**8**, **78**, **85**). Although the available data are not as complete or as exact as one might prefer, the implication that persistent cerebral anoxia may result in permanent damage to the central nervous system may be drawn. It is also probable that such anoxic insults may be cumulative, thus accounting for the fact that the anoxia produced by nitrous oxide anesthesia may be fatal in one subject while an anoxia of a similar degree has no effect on another patient. It must also be remembered that most of these clinical reports deal with rather marked symptoms which show up in terms of personality disorders or motor symptoms. It is highly probable that slight deterioration in intellectual capacities may pass undetected. Numerous problems remain for solution in this general field.

EFFECT OF INCREASED AIR PRESSURE ON MENTAL PERFORMANCE

Paul Bert (**12**), in 1878, carried out experiments in which animals exposed to increased air pressures went into convulsions and frequently died. These effects were attributed to the direct toxic action of oxygen in high concentrations. Experience has shown that men who have been working in compressed air, either in diving suits or caissons, are liable, upon their return to atmospheric pressure, to show a variety of symptoms, variously classified as " the bends," " caisson disease," or " compressed air illness." These symptoms are due to the fact that gas, chiefly nitrogen, which goes into solution in the blood and tissues during exposure to compressed air, is liberated in the form of small bubbles if too rapid decompression occurs. The resulting local or general blockage of the circulation may produce a variety of symptoms, depending upon the point at which the block has occurred. Various mental symptoms have been reported subjectively by investigators in this field. Shilling and Willgrube (**109**) report that men exposed to increased air pressure of 5 atmospheres or above have a definite feeling of stimulation and well-being which they liken to a feeling of drunkenness. At high pressures they have an exaggerated confidence in their ability to accomplish a given task, but to the observer their actual accomplishments fall far short of that demonstrated at atmospheric pressure.

Phillips (**88**) reports a similar failure of accomplishment, associated with emotional disturbances. Behnke, Thomson, and Motley (**11**) have published introspective reports of reactions under 4 atmospheres of pressure. The evaluation of the psychic changes was made on the basis of subjective reports. These authors report that visual, auditory, olfactory, and tactile reception were not affected, but the response to these stimuli was delayed. Recollection required greater effort and concentration was quite difficult. Frequent errors in arithmetical calculations were reported, and increased difficulty in laboratory manipulation was found. The retardation of mental activities was reported first at 3 atmospheres; at 4 atmospheres it is a slight handicap; and at 10 atmospheres it may render the individual helpless. Since the breathing of pure oxygen at pressures of 1 to 4 atmospheres, which would correspond to the same oxygen tension in air at 10 atmospheres, does not induce the psychic responses of air under these high pressures (**10**), the authors conclude that the increased partial pressure of oxygen cannot be a significant factor in the etiology of the psychological changes. They attribute the effects to the narcotic activity of the nitrogen, which in these high pressures behaves like ether or nitrous oxide, showing a high coefficient of solubility in lipoid matter. Their calculations show that in an air pressure of 10 atmospheres the concentration of nitrogen in the blood is equivalent to the anesthetic concentrations of nitrous oxide at ordinary pressures.

Behnke, Forbes, and Motley (**9**) have shown that oxygen at a pressure of 3 atmospheres can be breathed by healthy men for 3 hours without disturbing symptoms. During the fourth hour, however, a progressive contraction of the visual field, with dilation of the pupils and some impairment of central vision, is the most constant criterion of oxygen toxicity. From these experiments the authors infer that the oxygen acts directly on nervous tissues with a toxic effect. Although the observations reported above are of considerable interest, they do not afford any quantitative estimate as to the extent of impairment of mental performance.

Schilling and Willgrube (**109**) used arithmetic computation, cancellation tests, and light-to-touch reaction time as indices of mental performance in a group of 46 subjects who were subjected to increased atmospheric pressure. The results showed a steady increase in the time required for completing the arithmetic problems as the pressure increased from 100 to 300 feet in depth. Unfortunately, the arithmetic tests were quite short, affording only 19 possible errors, so that the discrimination in these experiments is not as great as is desirable. The reaction time was definitely increased at the higher pressures. Interestingly enough, it was found that, with experience, the degree of mental impairment at a given pressure was decreased. This effect of training was not studied in detail, although it is a subject which deserves attention.

Thus, with respect to introspective reports and quantitative, even though somewhat inadequate, measurements, the conclusion is justified that with increased oxygen tension and air pressure there is a slowing of mental and neuromuscular responses. Because of the difficulties in isolating the pressure effects from the effects of increased oxygen and nitrogen content of the inspired air, no completely satisfactory explanation of the mental effect exists. Phillips, for instance, states that the psychological changes encountered are due to purely mental and not to physical causes. He describes several cases of failure of accomplishment under pressure, which, through psychoanalytic procedures, were shown to be due to claustrophobia. However, the quantitative measurements of Schilling and Willgrube indicate a reduction in mental performance which cannot be attributed to a purely mental reaction such as claustrophobia.

Behnke, Thomson, and Motley (11) have stated that the psychological changes are due to the narcotic effects of the increased nitrogen tension. However, this is probably not the whole story, since it is reported that the greatest change is noticeable immediately after reaching a given pressure and lessens as the subject becomes adjusted. If the cause were nitrogen narcosis, the psychological difficulties would increase, rather than decrease, with the exposure. The direct toxic action of high concentrations of oxygen on neural tissue is the hypothesis which needs further experimental work. The question whether the psychological effects may be attributed to physical pressure changes has not been separated from the chemical effects of either oxygen or nitrogen. Another explanation of the apparent detrimental effects of increased oxygen content may be the possible increase in carbon dioxide of tissues (107). At the high pressure enough oxygen can be taken up in physical solution in the blood plasma to supply metabolic needs, so that the oxyhemoglobin does not become reduced in the tissues. Since oxyhemoglobin is a stronger acid than reduced hemoglobin, the basic ions are not released for combining with carbon dioxide in the tissues as is ordinarily the case. Thus, the carbon dioxide may accumulate in the tissues and central nervous system with detrimental effects on performance.

ACID-BASE EQUILIBRIUM AND MENTAL PERFORMANCE

The acid-base balance of the blood is maintained within fairly normal limits in a normal person by the interaction of the respiratory and excretory systems. However, displacements in blood acidity may be produced rapidly by the addition or removal of carbonic acid gas by way of the lungs. When the carbon dioxide content of inspired air is increased, the equilibrium is shifted toward the acid side. Conversely, when carbonic acid gas is removed from the blood by excess ventilation of the lungs, the equilibrium is shifted toward the alkaline side. These changes may take place rapidly over a period of minutes and result primarily in a shift in the acidity or hydrogen-ion content of the blood. Changes in the bicarbonate concentration of the blood may also be effected by the ingestion of acid or alkali. Such changes do not affect the hydrogen-ion concentration as much, and take place more slowly than changes affective through the respiratory system. Certain relationships between alterations in the acid-base equilibrium and mental performance have been reported. For instance, Gellhorn and his associates have shown that sensory discriminations which are mediated by the cerebral cortex, such as auditory acuity, visual intensity discrimination (**31**, **37**), and visual after-images (**38**), were greatly decreased both by increase and decrease in carbon dioxide content. Other psychic processes, such as formation of associations (**36**), cancellation, addition (**35**), etc., were interfered with under the same conditions. On the other hand, physiological processes involving the lower parts of the brain and not the cortex were enhanced when an excessive amount of carbon dioxide was removed from the blood by hyperventilation (**114**). For instance, vestibular nystagmus, produced in man by caloric stimulation, was increased under hyperpnea, whereas with excess carbon dioxide in the inspired air a decreased number of nystagmic movements was observed (**39**). Since carbon dioxide acts as a vasodilator on the peripheral blood vessels, but stimulates the vasomotor centers in the mid-brain to produce vasoconstriction, Gellhorn explains the impairment of psychic functions in terms of a lowered oxygen supply to the central nervous system.

Although it is possible to explain the deleterious effect of hyperventilation in terms of oxygen deficiency in the cerebral cortex produced by the vasoconstriction which results from the carbon dioxide deficiency, such a line of reasoning will not suffice to explain any detrimental effect of carbon dioxide excess. The detrimental effects of excess carbon dioxide are then attributed to its acid properties. Since carbon dioxide concentration below 3½% to 4% did not produce measurable effects, it is possible that the stimulating effect on the respiration may have interfered with the actual measurement of sensory threshold when used at the higher concentrations where respiration is markedly enhanced. The symptoms of increase in neuromuscular excitability resulting in unconsciousness and tetany have been reported by numerous investigators following hyperventilation (**20**, **43**). Since the acid-base balance of the blood returns to normal values within 1 or 2 minutes after cessation of hyperventilation, although the mental and psychological effects may persist for ¾ to 2 hours (Shock and Hastings, **112**), it is highly probable that the mental effects are the result of cerebral anoxemia rather than the loss of carbon dioxide itself. The cerebral anoxemia could be produced by cerebral vasoconstriction following the decrease in carbon dioxide in the blood.

The effect on mental performance of shifting the acid-base equilibrium by administration of acids or alkalis has been studied by Shock (**110**). Although significant displacement of the acid-base equilibrium could be produced in either an alkaline or acid direction, significant differences in mental performance, as measured by mental multiplication, were not observed. The negative results of this experiment might have been due to the fact that the experimental subject used had become so proficient in mental multiplication through practice that the test no longer represented mental effort for him. New experiments need to be repeated using a more sensitive index of intellectual capacity and a larger number of subjects.

EFFECTS OF DRUGS

It is practically impossible to make any adequate generalizations with respect to the effects of drugs on mental performance because of the wide differences of experimental technique, dosages, kinds of animals, and conditions under which experiments were carried out in different studies. Because of limitations of space, only those drugs which are apt to be encountered in everyday existence and on which experimental studies have been made will be considered.

Alcohol

The quantitative experiments on the psychological effects of alcohol are limited to studies which deal with the effect of alcohol administration on the performance in a mental function which has already been learned. The general criticism which can be applied to almost all these studies is that no account is taken of individual differences in absorption. Because of such individual differences, the only true index of changes produced in the internal environment is a chemical determination of the amount of alcohol in the circulating blood. In the studies reviewed, McFarland and Barach's (**74**), McFarland and Forbes's (**75**), and Fleming and Goldman's (**29**) are the only ones in which such an adequate control was made. Most of the studies which attempt to determine the effects of varying dosages of alcohol on mental performance are quite meaningless because no account of body size is taken and because of the wide individual differences in the rate of absorption and elimination.

The experimental evidence which tends to show an impairment of memory and judgment after administration of alcohol is summarized in some detail by Miles (**79**). In most of the studies reported, the impairment measured is small and is often of questionable statistical significance. Occasionally, individual subjects actually show improvement in some functions (Miles, **79**). Cattell (**17**) compared the intelligence test scores of 25 men and 25 women without alcohol with their performance on equivalent tests after doses of 10 and 20 gr. of absolute alcohol. Five equal tests of about 110 items were made up and used on 5 experimental days. The experimental procedure was such as statistically to counterbalance extraneous factors. It was found that the 20-gr. dose of alcohol produced only a 1% decrease in intelligence, while the 10-gr. dose produced no measurable change. These results are in accord with the previous study by Hollingworth (**55**), who employed a test of logical relations as opposites and found in the first hour after ingesting 39 c.c. of alcohol (absolute) a decreased efficiency of 15% as compared with normal performance. Administration of 79 c.c. of alcohol (absolute) gave a reduction of 25% under similar conditions. Seward and Seward (**106**) studied the effects of alcohol administration on a group of 12 male students, using, as a test of judgment, accuracy and reproduction of syllogisms of 1, 2, and 3 premises followed by a conclusion. After an administration of .75 gr. of alcohol per kilo of body weight, no effect upon judgment could be determined, although an impairment of immediate reproduction was evident, especially at increasing levels of complexity of the problem. The increase in errors after alcohol administration was statistically reliable, as was the increase in reading time observed. Fleming and Goldman (**29**), in a carefully controlled study, could find no effect of small doses of alcohol on the average mental reactions time. In this study, the test in mental addition was applied at 10- to 25-minute intervals after the administration of the alcohol. Furthermore, the concentration of the alcohol in the blood was determined on each of the 17 subjects. Similar experiments should be carried out in the same way, using dosages greater than the .6 to 1 c.c. of absolute alcohol per kilo of body weight used in this study.

McFarland and Barach (**74**) report impairment of mental performance as measured by choice reaction, color naming, pursuitmeter, batting, and a code test after administration of ¾ gr. of alcohol per kilo of body weight. Actual concentrations of alcohol in the blood were determined at the time the tests were given. When 50% oxygen and 3% CO_2 were breathed, the amount of alcohol present in the blood was reduced, and the impairment in the mental test was significantly less in 4 out of 5 subjects tested. McFarland and Forbes (**75**) found that alcohol produced a greater impairment in auditory threshold at high altitudes than the same dosage produced at sea level. They also showed that the concentration of alcohol in the blood rose more rapidly and reached a higher level at high altitudes than at sea level. Thus, the amount of oxygen available is an important factor in the rate of elimination of alcohol from the body.

From experimental evidence available, we may thus conclude that higher mental functions show impairment when subjected to quantitative measurement, although the relationship between the amount of alcohol circulating in the blood stream and the degree of mental impairment is still uncertain.

Aspirin

When aspirin was administered in medium analgesic doses no measurable effects on mental functions, such as addition, code translations, or opposite tests, were found by Davis (**24**). Similarly, aspirin in 10-grain doses, administered 30 minutes before the test, had no effect on the rate of learning lists of 12 syllables when measured in terms of number of trials necessary for learning (Jones, **63**).

Tobacco

The experimental results on the effect of smoking on mental performance are difficult to summarize because of the different viewpoints and aims of different experimenters. Hull (**59**) attacked the problem primarily in terms of the physiological changes induced by the absorption into the blood stream of substances from the tobacco smoke and took great precautions to exclude psychogenic factors by administering warmed air as a control dose. In other studies, as, for example, that by Fay (**28**), the smoking situation has been regarded as a variable, and no attempt was made to set up control periods under standardized dosage and other physical factors. We are also confronted with studies of the effects of smoking on performance in learned habits as contrasted with studies aimed to test the effect of smoking on the establishment of habits. In addition, differential effects between smokers and nonsmokers are reported. In general, the amount of impairment in learned mental functions is slight and of questionable significance even in nonsmokers (Hull, **59**; Fay, **28**).

Fay found that nonsmokers reacted more slowly to red light for about 5 minutes after smoking began, while regular smokers were little affected. Hull found that mental addition was slightly impaired in nonsmokers, but that with habitual smokers a favorable effect was produced. Fay reported that regular smokers tended to react more rapidly to a choice between red and blue lights for 1 hour after smoking 1 cigarette, while nonsmokers were little affected. He reported large individual differences and wide fluctuations in the reaction time, so that the significance of this finding is questionable. For instance, each subject was given 1 practice session, and the reaction time was taken as the average of 10 trials. It is doubtful whether such a small series would render an adequate index of reaction time for comparative purposes.

Pechstein and Reynolds (**87**) exposed rats to tobacco smoke over a period of 30 minutes to 3 hours daily for 30 to 60 days. The animals were exposed to smoke from 5 gr. of tobacco, so there is no indication whether the effects were produced by nicotine, carbon monoxide, pyridine, or some other combustion products of tobacco. After fuming, the animals were transferred to the maze, and learning curves were obtained. It was found that rats fumed with a limited amount of tobacco smoke excelled all normal and experimental groups in maze learning. Animals who were fumed over longer periods of time were less effective in learning, with greater variability in the female group. The authors concluded that tobacco smoke to a small degree acts as a stimulant and enhances learning, while excessive smoking depletes learning capacity to the point of inability by the fourth generation.

Using a similar technique for exposing rats to varied degrees of tobacco smoke, Phillips (**89**) was unable to find any statistically reliable difference between experimental and control groups of animals with respect to maze learning ability. In this experiment, groups of animals were placed together in the smoking compartment, so that it is possible that some of the harmful ingredients of the smoke may have been absorbed on the hair of the animal and not inhaled. Furthermore, the animals tended to huddle, which might well have reduced the intake of noxious fumes in the animals at the bottom of the pile. In this experiment, a continuous flow of air was drawn through the smoking chamber so that concentrations of the smoke in various points of the chamber might well have been widely different. In these experiments, as in those of alcohol, analyses of the amount of nicotine or carbon monoxide circulating in the blood of each animal should be made for accurate quantitative work.

Caffeine

Caffeine, when administered as the raw alkaloid or as caffeine citrate, tends to reduce simple motor sensory reaction time from 6 to 8%, depending upon the individual subject (**108**).

Cheney (**18**, **19**) reported that reaction time is reduced more by coffee than by an equal amount of caffeine administered as free alkaloid. Maximum reduction in reaction time occurred between 1½ and 2 hours after administration of caffeine. Reaction time was not affected by a single dose of 3 mg. of caffeine per kilo of body weight. Doses of 3 or 4 mg. per kilo of body weight had a variable effect, while 5 mg. caused a decrease in reaction time over a 3-hour period. Twenty-four hours later the effect had entirely disappeared. Horst and Jenkins (**57**, **58**) reported that 3 to 4 mg. of caffeine per kilo of body weight will shorten reaction time by 2 to 6%. They found that in some of their subjects reaction time was lessened 24 hours after administration of the caffeine. Switzer (**116**, **117**) administered 5-grain doses of caffeine citrate to a group of 25 subjects. Four hours after the drug was given each subject received 16 reinforced conditioned stimuli consisting of a light. From these experiments he concluded that caffeine tends to diminish the inhibitory responses, since the latent period of the conditioned reflex was definitely shortened under the influence of caffeine. The data also tend to show that caffeine augmented the unconditioned reaction of the galvanic skin reflex to electrotactile stimulation. The differential effect of dosage in the case of caffeine is illustrated in a study by Hollingworth (**54**), who found that the speed of performance at typing was quickened by doses of from 1 to 3 grains of caffeine, although it was retarded by larger doses of 4 to 6 grains. Small doses of caffeine seemed to increase speed and decrease errors in typing, although large doses of greater than 6 grains had the opposite effect. Hull (**60**) administered 5 grains of caffeine citrate to each of 8 male subjects. Three and one-half hours after the administration of the citrate, each subject learned a list of 16 syllables, which were carefully standardized and equated. Although caffeine was found to be associated with an increase in anticipatory displacement in reaction during the learning process, there was a mean reduction in the rate of learning amounting to only 2 to 3%, which was not statistically reliable. Similarly, learning done under the influence of caffeine was relearned slightly more easily than that done after taking a control dose of lactose, but this tendency was not statistically reliable. This experiment was especially well controlled and executed and seems to indicate that caffeine has no significant effect on learning. Cattell (**17**) administered equivalent forms of standard intelligence tests to a group of 25 males and 25 females, mostly of college age, after they had been given .2 or .4 gr. of caffeine citrate. Even with the smaller dosage of .2 gr. of caffeine there is a slight lowering of average IQ in the group, although the difference is not statistically significant. On the other hand, scores on a memory test consisting of 30 items, which was administered at the same time, improved when caffeine citrate was taken.

The author is of the opinion that there is some indication that men are more susceptible to caffeine and less susceptible to alcohol than women, although the statistical evidence is inadequate. Systematic investigation of the effects of caffeine administered at different stages of the learning process is needed.

Benzedrine

Skinner and Heron (**113**) have shown that administration of benzedrine will cause restoration of a conditioned reflex which has passed through a period of extinction. Twelve white rats were trained to press a lever to receive a food pellet at 4-minute intervals. They found that under standard conditions the rate at which the animals pressed the lever was fairly uniform, even though a food pellet was discharged only every 4 minutes. When the food reward was removed, the rate of pressing the lever diminished. On the eleventh day of extinction ½ mg. of benzedrine sulphate caused complete restoration of the rate of response of the conditioning period. Other experiments have shown that injection of benzedrine actually reduces the hunger drive (**119**), so that the above effect is attributed to the increased motor activity produced by the drug. The problem of why these motor activities find expression in terms of the previously conditioned response is still obscure.

Bahnsen and Thesleff (**2**) were unable to establish any significant differences in reaction time after administration of benzedrine. The number of reactions reported was insufficient to establish a stable mean for each subject, so that the results are of little value.

Sargant and Blackburn (**99**) administered Cattell's intelligence tests to 25 mental patients before and after administration of 20 mg. of benzedrine. A control group which had received a blank tablet in place of the benzedrine showed no change in average score, while the experimental group showed a rise of 3.9 points, or 8.7% in the average score. The author interprets this increase in score in terms of the influence of nonintellectual factors, such as relief from anxiety, mild depression, etc. Molitch and Sullivan (**82**)[2] tested 96 boys between the ages of 10 and 17 with the new Stanford achievement test. A week later, 50 of the boys were given 10 mg. of benzedrine; the remainder received a blank tablet; all were retested. Twenty-six of the boys, who either lost or failed to improve on 10 mg. of benzedrine, were then given 20 mg. and were retested. Although 8.6% of the controls showed a gain in their scores, the group average dropped 29 points on the retest. Thirty-two per cent of the boys showed a gain on 10 mg. of benzedrine with an average of 63 points, while of the boys who were tested a third time after having been given 20 mg. of benzedrine 92.3% gained, on an average, 117 points in their scores. When benzedrine is administered to normal individuals, reports of well-being, less fatigue, and increased mental efficiency are usually obtained, although Myerson (**83**) found that in 2 out of 10 cases drowsiness and sleepiness followed benzedrine administration. For instance, Nathanson (**84**) administered 20 mg. of benzedrine per day to each of 55 normal persons in single test days. Sixty-two per cent reported less fatigue, 56% noted increased talkativeness, while 54.5% reported increased capacity for work. No such reports were obtained from a control group of 25 subjects who received similar tablets containing calcium lactate. From these experiments the author concludes that the drug should be of value in the preparation of individuals for activities that require unusual expenditure of physical and mental energy.

Gwynn and Yater (**45**) administered 20 mg. of benzedrine per day to each of 147 medical students over a period of 3 days. A control dose of lactose was administered. After administration of benzedrine 113 of the group reported that they felt peppy; 72 reported that they became exhilarated; 42 reported that they were more talkative; 61 reported greater powers of concentration; 126 suffered from insomnia. No tests of actual performance were given. Similar stimulating effects, based on subjective reports of 10 normal persons, are reported by Davidoff and Reifenstein (**23**). In comparing these results with those obtained with the administration of benzedrine to psychotic patients the authors conclude that benzedrine is apparently more stimulating to normal persons than to depressed or self-absorbed patients, and that it is more effective in increasing motor activity, speech activity, and general efficiency than in elevating the mood.

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[2] The same data are presented by Molitch and Eccles (**81**).

The stimulating effect of benzedrine on motor activity is confirmed by Zieve (**122**), Searle and Brown (**105**), and Brown and Searle (**14**) in their studies on rats. Brown and Searle found that when the drug was given every fourth day to a group of 15 white rats, the spontaneous activity of the animals was markedly increased, with a maximum in the first hour after the injection, falling off rapidly to normal or subnormal values within 4 hours. Doses of 3 mg. per kilo gave greater stimulation than did 1½ mg. per kilo or 4½ mg. per kilo. of body weight. Bradley (**13**) administered 20 mg. of benzedrine sulphate daily to each of 30 children between the ages of 5 and 14 who were referred to a clinic for behavior problems. The intelligence of the group was, in general, within normal limits. During a week of daily administration of benzedrine 50% of the children showed a marked improvement in school performance. They report that the effect appeared within 45 to 50 minutes after the benzedrine was given, reaching a maximum during the second or third hour and disappearing within 6 to 12 hours after. Schube, McManamy, Trapp, and Myerson (**104**) administered benzedrine to 80 patients from the psychiatric clinic of the Boston State Hospital. The subjects were distributed among all psychoses and were given 10 to 30 mg. of benzedrine daily for 30 days. The authors found no improvement in any of the cases studied, and in 15 cases a temporary accentuation of the psychosis. Similar reports are given by Davidoff and Reifenstein (**23**), who found increased motor activity, increased speech response, and improved general efficiency in a group of dull, self-absorbed, negativistic, shy, reclusive, inactive, and resistive mental patients after 3 weeks' treatment, but little effect on mood.

The subjective reports of increased mental efficiency given by normal subjects after the administration of benzedrine are not borne out by quantitative measurements of output in the experiments of McNamara and Miller (**77**), who found that the number of problems of written multiplication of two 3-digit numbers was not materially increased nor were the errors affected even though subjective feelings of stimulation were reported. Barmack (**7**) found that administration of 10 mg. of benzedrine to each of 36 students increased the number of problems of adding pairs of 6-place figures attempted, but had no effect on errors.

Although most experimenters report increased feelings of well-being and increased motor activity in normal persons, results from psychotic patients failed to establish any marked alterations in mood. Consequently, the conclusion of Sargant and Blackburn that the improvement in intelligence test scores is due largely to nonintellectual factors is open to question. More experiments in which quantitative measures of mental output are used in normal subjects, including children, are highly desirable.

Phosphate Administration

The interesting possibility of inadequate dietary intake of inorganic phosphate contributing to fatigue, lassitude, and lowered intelligence of school children is raised by experiments carried out in Germany. During the War Embden (**27**) reported that he had experienced a mental freshness and euphoria, at a time when he usually felt sleepy, by the ingestion of moderate amounts of primary sodium phosphate or recresal. Poppelreuter (**90**) made the first systematic investigation to show the effect of phosphate administration on mental activity. He used 2 groups of experimental subjects who carried out continuous additions for periods of 3 to 5 hours. The author claims that without exception the intake of recresal resulted in an increased quantity of work as well as a decrease in the number of errors. Unfortunately, this study was inadequately controlled in terms of the effects of suggestion, since it seems likely that the purpose of the test was soon discovered, especially by the 5 psychotechnicians who served as subjects.

Griesbach (**44**) studied the effect of recresal administration on multiplication and division work in a group of bank clerks. No significant differences were found resulting from recresal feeding. Dahmen (**22**) attempted to find out whether there were any differences in the effect of recresal when it was used continuously and when it was given in a single dose. His test consisted of tachistoscopic perception, a mental activity which seems particularly ill-advised since it is practically free of mental work. In no case did he find a decrease in efficiency after recresal administration. In 2 cases the results were doubtful and in 4 cases, favorable. Since no control tablet was used, the factor of suggestion was not excluded. Since the mental test chosen was ill-advised, the results of this experiment are of little value.

On the other hand, Rahm (**96**) carried out an extensive experiment under very rigorous control but could find no evidence of an increased mental efficiency with the administration of recresal to adults. In the case of children, single doses of recresal failed to show any augmentation in mental performance. However, after recresal had been taken for a number of weeks an increased efficiency was observed in mental activity as measured by mental addition, preassociation, and a cancellation test. In control experiments he found that the effects of suggestion were of considerable magnitude. It was found that, after the proper suggestion, both recresal and the sham drink produced an increase in efficiency. After the intake of recresal the performance in general was not greater, but often even less, than after the intake of the sham drink. On the other hand, the intake of caffeine showed increases in efficiency that were considerably greater than those obtained in the suggestion test. Since it is known that children are by far more suggestible than adults, it is possible that the beneficial effect of recresal which is reported for children was primarily an effect of their greater suggestibility.

Strauch (**115**) reports an improvement in school work and general health in a group of school children following the administration of small doses (1 grain) of monosodium phosphate or recresal with each meal. No standard tests or control groups were tested, so that the scientific value of his experiments is not great.

In summarizing the experimental results of phosphate feeding on mental performance we may say that objective proof of its stimulating effect is still lacking but that the tendency for positive results to appear with children is interesting and requires further controlled experiments.

CARBOHYDRATE METABOLISM AND MENTAL PERFORMANCE

Since it has been shown that the only source of energy available to the central nervous system is in the form of carbohydrates (**49, 50, 51, 56, 66**), the correlations between lowered blood sugar levels and mental performance are of interest. Hoelzel (**53**) reports increased mental efficiency after short fasts which he correlates with low values of blood sugar (0.43 mg. per cent). These subjective reports are not in accordance with the objective tests administered by Glaze (**41**), who found a decrease in all mental tests in his study of fasting. As yet, no controlled laboratory studies exist to show a relation between blood sugar level and mental performance in humans, although the speculations of Barcroft (**6**) are suggestive.

Experiments with animals have shown that, when the blood sugar level is reduced slightly by small injections of insulin, conditioned reactions are augmented. Heron and Skinner (**48**) trained rats to press a lever to obtain food. One hour after the injection of .02 unit of insulin the time required to establish the habit was reduced by 31%. In a similar experiment, using the same type of problem and apparatus, Wentink (**119**) found a reduction of 26% in time required 1 hour after injection of .005 unit of insulin in rats. It is quite probable that this augmentation in learning is more the effect of increasing the "hunger drive" in the animal than of change in glucose available to its central nervous system, and it has been shown repeatedly (**16, 25, 95**) that small doses of insulin produce hypertonus and hypermotility of the stomach, which is the physiological basis of sensation of hunger. Since wide individual differences in sensitivity to insulin exist, it is important that blood sugar values be obtained in order to draw conclusions in such experiments. Furthermore, the maximum effect of insulin on blood sugar usually occurs shortly after intravenous injection. In both the above experiments interpretation is further complicated by the fact that the insulin injections were subcutaneous, thus introducing a further variable of rate of absorption. A systematic investigation of the entire problem is needed.

Rose, Tainton-Pottberg, and Anderson (**97**) found that a series of convulsive administrations of insulin restored a conditioned response in sheep. The conditioned reflex of the sheep had been studied over a period of 7 years, but during the last year the reflex had gradually failed. Convulsions were induced on each of 7 successive days by the administration of insulin. During the period of insulin shock no reflex could be elicited, but 1 hour after administration of glucose by mouth the conditioned reflex, absent for almost a year, reappeared with abnormal vigor. The augmented reflex persisted over a period of 2½ months. No interpretation of this experiment is made by the authors in their preliminary report, but it is evident that the augmentation effect cannot be attributed to the immediate effects of lowered blood sugar, since during this period no response could be elicited. After restoration or augmentation of the blood sugar by glucose administration, the response was enhanced. Suitable controls to show that the effect cannot be attributed to the immediate rise in blood sugar were not reported, but it is presumed that the effect is produced by some long-term effect of the insulin shock. It is possible that the effects are due to some permanent damage to the higher centers resulting from low sugar and O_2 of the blood, with attendant removal of inhibitory effects of these higher centers, although more experiments are needed before satisfactory conclusions can be drawn.

One of the chief physiological effects of epinephrine is the increase in blood sugar which is produced by relatively small injections. Hence, it may be well to discuss the effects of injections of adrenaline on mental performance under this section rather than in connection with drug or endocrine factors.

Heron and Skinner (**48**), as well as Wentink (**119**), have reported that adrenaline caused a 25 to 30% decrease in rate of response in rats previously trained to a motor act as described in a preceding paragraph. The results are interpreted as due to removal of the hunger drive of the animals resulting from the inhibition of gastric motility following administration of epinephrine. However, Heron and Skinner report a short period of increased rate of response immediately following the injection, although no such period of increase was observed under similar conditions by Wentink. Thus, the results obtained are equivocal.

In these experiments, the habit was established, and the drug effects studied referred to alterations in motivation for the performance of the habit. Experiments on the effect of adrenaline on mental performance in humans have been carried out by Jersild and Thomas (62). In 6 subjects intramuscular injection of 7–16 minimum of 1:1000 solutions of adrenaline chloride decreased performance in substitution, free association, mental calculations, and color-naming tests, although speed of tapping and muscle strength were improved.

The effect of adrenaline on learning ability has been studied in some detail by Akimov (1), who reported an accelerative effect on maze learning in rats. With food as the motivation, the augmentation in learning is the more surprising, since hunger was presumably reduced by the injection. Groups of 9 to 18 animals were tested under different conditions, and it was found that ½ c.c. of 1:500,000 adrenaline, beginning on the eleventh day of learning, increased the learning rate more than when adrenaline was administered from the first or sixth day of learning. Adrenaline 1:500,000 was more effective in stimulating learning than was adrenaline 1:100,000 or the administration of electric shock. The control group of animals which received daily injections of normal saline did not learn as quickly (measured by trials or errors) as those receiving adrenaline injections, but was significantly better than the control groups which received no injections. The author attributes the improvement to increased tactile sensitivity in the rat as an aid to his learning. However, a more plausible explanation is that the emotional stimulus of receiving an injection, even of saline, stimulated the animal to release adrenaline from its own adrenal glands. Hence, we must attribute some synergistic influence of slight doses of adrenaline to the learning process. This study is one of the few reviewed in which any assessment of the effect of time of injection and dosage on learning was made.

The hypothesis that mental diseases may be due to cerebral malnutrition resulting from hyperinsulinism has been an attractive one (Harris, 46, 47; and Powell, 91, 92, 93, 94), although such a theory has yet to receive adequate confirmation.

Powell (94) reported an interesting relationship between blood sugar levels and mental performance in a group of 3 patients. In a girl of 12 years of age Powell found that mental performance as measured by school work in spelling was markedly improved when the blood sugar level, which in most tests hovered around 50 mg. per cent in this patient, was raised to approximately normal value of 120 mg. per cent by the frequent feeding of high-carbohydrate food. Maximum results were obtained when the school tests were given at the time when the blood sugar was high. The same spelling test, given 2 hours later when the sugar level had dropped back to the low value of about 60 mg. per cent, shows a definite decline from the score when the blood sugar was elevated. The cause of hyperinsulinism is obscure, but the author tended to attribute it in part to the high sugar consumption and the result of continued stimulation of the pancreas. Interesting as this case may be, more experiments with a rigorous laboratory control and standardized procedures for testing mental performance are necessary before a final judgment can be passed.

Considerably more information is available with regard to the long-time effect of disordered carbohydrate metabolism. Joslin (**64**) stated that " one-third of our diabetic children are of superior intelligence." This statement is based on the results of giving the Stanford-Binet test to 169 diabetic children.

A detailed study of the same 169 diabetic children was made by White (**121**) in Joslin's clinic. An intelligence quotient above 110 occurred in 23%, of from 90 to 110 in 54%, and below 90 in 13% of the children. Seventeen children from 1 to 6 years of age were tested with the Binet test, and in this group, 9, or 53%, had an IQ from 90 to 100; 2, or 12%, had an IQ below 90; and 6, or 35%, had an IQ above 110. The range was from 84 to 143, with a median of 108. Mental deterioration occurred in 4 of the diabetic children studied, all with onset in infancy; but in 2 of these cases definite cerebral trauma from infection occurred. From these results White concluded that in the period of life in which mental growth was increasing at a rate far exceeding that of the older child, the diabetic had better than the average intelligence, proving that diabetes is not a handicap to mental progress.

Miles and Root (**80**) administered a series of psychomotor tests, such as mental addition of numbers and cancellation of digits, to a group of 40 adult diabetic patients and compared the results with a control group of adults of the same age. They found that diabetic patients frequently complained of poor memory and power of attention but failed to find objective proof of any deficiency. However, diabetic patients with hyperglycemia and glycosuria at the beginning of treatment showed a decrement of about 15% or more in memory and attention tests. With treatment, the diabetic improved rapidly in his psychological status, approaching, but not quite reaching, normal. West, Richey, and Eyre (**120**) administered the Stanford-Binet test to 76 juvenile diabetics and found that the IQ of the diabetics showed a somewhat higher average than that of normal children, although there was considerable variation. Improvement in diabetes did not raise the IQ, and when the diabetic control lapsed the IQ tended to remain unchanged or to rise. Since a socioeconomic selective factor may operate when diabetics under treatment are studied and compared with the average population, the previous studies need a more adequate control of this factor. Such a study has been reported by Brown (**15**), who compared mental-test data of 60 diabetic children with similar test data obtained on 28 siblings of the diabetics. In order to minimize the possibility of subsequent development of diabetes in the sib controls, the next older rather than the next younger child to the patient was chosen for the control group. It was found that the group of diabetic children was distributed quite normally as to intelligence, with 43% of the diabetics within the IQ-range of 90 to 100 and approximately equal numbers above and below. Both the mean and the median IQ are almost exactly 100, as compared with a median IQ of 106 for the siblings. No relationship was found between duration or severity of the disease on the one hand and intelligence on the other. School achievement and behavior of the diabetic group compared favorably with that of their siblings in spite of poorer school attendance. It is suggested that the high percentage of bright children in Joslin and White's (**121**) diabetic group may be due in part to the high percentage of Jewish children among their cases and in part to the selection of those patients from the upper socioeconomic groups.

From these studies we may conclude that objective evidence for the superiority in intelligence of diabetic children is lacking.[3]

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[3] A brief summary of the literature on endocrine factors in mental growth and development has been recently published by Shock (**111**). A more complete review of the influence of endocrines on mental performance is in preparation.

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INDEXING THE *PSYCHOLOGICAL INDEX* [1]

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AND
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W.P.A. Staff

A brief note in a recent number of the *Psychological Bulletin* referred to the "Psychological Index Project" conducted under the Works Progress Administration in New York City. As this project has developed into one of large proportions, and is one which is expected to benefit psychologists everywhere, it seems fitting to give a brief account of its history, its present status, and its probable eventual outcome.

I. History

The difficulty of locating references in the *Psychological Index* in the absence of anything but a very general classification of subjects in each volume, contrasted with the great convenience of a cumulative author index of all volumes maintained in the Department of Psychology at Columbia University (and probably in certain other universities), inspired one of the authors years ago to entertain the idea of a cumulative *subject index* covering all volumes of the *Psychological Index*.

The need for such an index was felt keenly by C. M. Louttit while he was preparing, with the support of the National Research Council, his *Bibliography of bibliographies* (1928). Shortly thereafter, a plan for a cumulative index began to take form in his mind, and in the spring of 1933 he conferred with one of the writers, then the Chairman of the Division of Anthropology and Psychology of the National Research Council. The idea of such an index was discussed from time to time with many psychologists, editors, and publishers. Estimates of the magnitude of the task, ways in which it might be worked out, and the probable cost were obtained from various interested persons during 1933. It appeared from preliminary surveys that if the actual indexing were to be done without charge by psychologists, the total cost of the project including printing would be about $25,000, and the probable returns from sales would be about $7,000, leaving a balance of $18,000 to be provided. In spite of the need for such a large subsidy, the proposal met with enthusiastic and almost unanimous support. It seemed that the next step should be the presentation of a fairly detailed proposal to the American Psychological Association. This was done in the fall of 1934.

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[1] This work is financed and operated by the Works Progress Administration of the City of New York, Professional and Service Division, as part of Official Project # 465-97-3-18, Bibliographies and Indices of Special Subjects.

The Council of the Association acted upon the proposal by referring it to the Board of Editors of the Psychological Review Publications for report at the 1935 meeting of the A.P.A. The Board of Editors, in turn, appointed the following committee of 3 to study the proposal: W. S. Hunter, Chairman; H. S. Langfeld; and C. M. Louttit. This committee reported to the Board of Editors in March, 1935, the result of a canvass of psychologists in 21 universities, showing 101 for, 36 against, and 28 doubtful. After considering the probable limited distribution of the Index in the form in mind at the time, and the probable cost of editorial work, manufacture, and distribution, the Editorial Board presented the following recommendation to the Council of the A.P.A.:

> "That it is the opinion of the Board of Editors and their advisors that, in spite of the strongly favorable vote, it would be inadvisable on account of the large deficit to publish the cumulative index of the *Psychological Index*."

When presented by the Council to the business meeting of the A.P.A. in 1935, the resolution was adopted.[2]

Although the proposal was thus officially disposed of, for the time being at least, the further decision of the Association at the same meeting to discontinue publication of the *Psychological Index* with the 1936 number made the whole idea of a cumulative index seem more desirable than ever;[3] for it would then be possible to prepare a final and complete Index covering the entire 42 volumes, with never any need for revision or supplementation.

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[2] See Proceedings of the Forty-third Annual Meeting of the American Psychological Association, *Psychol. Bull.*, 1935, **32**, 649–663.

[3] The Association showed its continuing interest in the project, however, by appointing upon recommendation of the Council, at the 1938 Annual Meeting, an advisory committee to work with the project directors of the Indexing Project. The committee consisted of A. T. Poffenberger, K. M. Dallenbach, C. M. Louttit, and R. R. Willoughby. Although no funds were provided for meetings of this committee, the members have frequently been called on for counsel and assistance in determining policies.

II. PLANNING OF THE PROJECT

The matter rested thus until the fall of 1936, when representatives of the Works Progress Administration in New York City invited one of the writers to present an outline of a project that would give employment to a considerable number of unemployed psychologists, translators, research workers, and high-grade clerical workers, and at the same time have an outcome whose value could not be questioned. The Indexing Project was tentatively offered and accepted, as it seemed to satisfy these requirements, and particularly as it fitted conveniently into a program of bibliographic research (Bibliographies and Indices) which formed a part of one large white-collar division of the W.P.A. (Research and Clerical Division). Dr. Louttit, upon being informed of the successful negotiations with the W.P.A., expressed delight that his brain child should be adopted under such favorable auspices.

In conformity with W.P.A. regulations the original setup was as follows: Sponsoring Agency, New York State Psychiatric Institute (such projects must have a tax-supported institution as official sponsor); Coöperating Agency, Columbia University; a director; a project supervisor and assistant supervisor; 10 head research workers and 60 research workers and translators; 5 stenographers, 5 library clerks, and 3 typists. The detailed program to be formally presented to the Planning Section of the W.P.A. for consideration and approval was prepared by Dr. S. B. Sells, who was thoroughly familiar with the organization of such projects and who served as project administrator during the early stages of the program.

The original plan called for indexing the 107,226 references found in the *Psychological Index* from 1894 to 1928, inclusive (overlapping the *Psychological Abstracts* by 1 year), with an average of 2 index entries for each item. The indexing technique was to follow that used in the annual index of the 1936 volume of the *Psychological Abstracts,* except that any article would be located by volume and reference number in the *Psychological Index,* thus:

Stutterer, association in, group influence, 35:1640

meaning that the reference would be found in Volume 35 as reference number 1640. Minimum intelligence and educational requirements were specified for the different grades of workers, and minimum psychological training for those concerned with reading and classifying. Each worker was to go through a training period and be tested before actually beginning productive work.

Productive work did not get under way until June 18, 1937. In the meantime, many puzzling and troublesome problems had to be studied and overcome. Some of these called for radical changes and improvements in the program.

1. The most serious and persistent problem was the finding of competent workers who would at the same time be eligible for employment by the W.P.A. Although one of the original purposes of the project was to employ psychologically trained persons who were sorely in need of work, few of these seemed to have been reduced to such financial straits as to be actually on Home Relief rolls. Furthermore, it became clear very early that competent technical supervisors could not be found on the relief rolls. It was decided, therefore, to begin operations with a very limited staff in the hope of building it up gradually. On July 1, 1937, 19 persons were at work: 11 research workers and 8 administrative and clerical workers. It has been possible since that time to build up gradually an excellent staff with adequate special training and with competent supervision. The staff at present comprises 65 workers: 1 director, 2 assistant project supervisors, 2 psychologists, non-relief, 26 research workers, 16 translators, and 18 clerical workers.

2. The limited utility of the finished product was a matter of much concern during the period of planning, since the Index would have to be used in conjunction with the volumes of the *Psychological Index*. A search among cumulative indices in other fields suggested that reference might be made in each case directly to the source without making the printed document too cumbersome. Particularly encouraging was the *Index of Geological Literature of North America*, covering the period from 1785 to 1918, with both author and subject index, and printed in 1 volume of about 1200 pages. It appeared from our calculations that the whole subject index could be printed on about 1200 pages, 3 columns to the page, if the references were to be cited in some brief form such as the following:

Stutterer,
    association in
        group influence   J. Ab. Soc. Ps. **23**, 45 [4]

The project was, consequently, modified to conform to this new plan.

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[4] In order to save space, the shortest and most simple forms of citation of journals are to be used.

3. One of the earliest decisions concerning the technique of indexing was that it could not be adequately done from the title of an article alone. Some examination of the article itself would be needed. The original plan, therefore, called for the reading of an article by the research worker and the selection by him of the indexing cues. All research workers were to be given training and tests for proficiency in reading and indexing by having all of them read and index the same selected series of articles. These results were to be brought together and discussed with the group, pointing out inadequacies and showing the correct indexing. Preliminary results of this training were somewhat discouraging and led to a modified procedure as follows: The function of indexing was separated from that of abstracting. One group of research workers now read the article and prepared an abstract of it in accordance with instructions given in a 25-page abstractor's manual, supplemented by interviews. This abstract was to be sent on to trained indexers who would make the classifications, decide upon cross references, etc. The training given to the indexing group will be described briefly in (4).

4. The first intention to use the index number of Volume 10 of the *Psychological Abstracts* as the model for classification of the material was supported by opinions from the Library of Congress, the New York Public Library, and the H. W. Wilson Company. However, serious criticisms came from the editors of the *Psychological Abstracts,* who had developed its index and who pointed out numerous shortcomings in it. Preliminary indexing had also suggested that the system, though valid for 6000 references published within a year, would be quite inadequate for 107,000 references spread over 35 years. The whole question of a proper indexing system for psychological material was therefore reopened for systematic study and a more thoroughgoing system developed (see Section IV). This was prepared in the form of a 24-page manual which served as a textbook for training the indexers. All prospective indexers, 40 in number, were required to attend a study course of eight 6½-hour days at which 96 abstracts were read, indexed on the blackboard, and thoroughly discussed under the instruction of R. G. Wetmore. The course was concluded with an examination. The indexing staff of 13 was chosen from the persons standing highest in the examination.

5. The references contained in the *Psychological Index* undoubtedly vary widely in their importance and particularly in their significance for psychologists. In recognition of this fact the suggestion

has frequently been made that material from remotely related fields and material that is trivial be ignored in the preparation of the Index. This calls for an exercise of judgment for which the project director did not wish to take the responsibility. What is insignificant at one time may become significant at another, and what is only remotely related to our field at one period may, at another, come to be closely related. Moreover, it is often necessary to know the inadequate attacks upon a problem in order to avoid similar pitfalls in later work. Finally, there is something to be said for completeness if the final index is to be called an *Index of the Psychological Index*. It has been decided, therefore, to index all references, in the face of estimates of several psychologists that from 10 to 25% of the material could be disregarded.

III. PROJECT OPERATION

The operation of the project may best be presented by outlining the work flow by which a reference passes through the successive processes of preparation, assigning, abstracting, indexing, editing, typing, and filing. The present tense is used throughout, although the first phase has been completed and the last not yet begun.

a. *Preparation.* References cut from the pages of the *Psychological Index* are pasted on 3 x 5 cards, called work cards. The volume number is stamped immediately on the upper left corner of the card, above the reference number on the pasted slip.

b. *Sorting for Assigning.* Work cards are sorted by languages and, within each language, into journals and books. Library locations are stamped on the journal references. Books are looked up in library catalogs by clerks, who record on the cards the call letters and the library or building in which the book may be obtained.

c. *Assigning.* Work cards are assigned to research workers and translators by one man in accordance with their interests, abilities, and knowledge of languages. The assignment packs of about 80 cards are recorded (by volume and reference number) in the work-record book of the particular assignee and are held at the assignment desk until that worker's next scheduled office visit, when they are "dated out" and delivered to him with a record of their number.

d. *Abstracting.* After obtaining the original article or book at the Forty-second Street Public Library, Columbia University, Academy of Medicine, or Psychiatric Institute, the fieldworker checks the accuracy of the printed reference and writes a summary of the article on the back of the card

For the purposes of this project, a highly specific abstract or summary is necessary. A check (duplicate abstract) on about 4% of the abstracts submitted to date has shown a high reliability in the work. Fieldworkers have been trained to give content, not topic; to restate, not to characterize; to give what the author says, not what he talks about. For experimental articles, method (or apparatus or both), subjects, results, and conclusions are specified. Abstracts are noncritical and nonevaluative except in one case: "Although the author may be vague, your abstract should be clear and definite, even to the point of stating his vagueness and the precise issue on which he is vague" (*Abstr. Manual,* p. 19). Discrepancies between title and content are clearly indicated.

About one-third of the assigned cards bear a notation indicating that a published abstract will be found in, for example, *Arch. f. d. g. Psychol.* Ansbacher's idea and method of utilizing these already available summaries has resulted in a considerable saving of time, particularly in the case of summaries of foreign books and articles. Plans are now under way for the publication of lists of published abstracts either as a volume of about 150 pages or in such form as to permit their insertion in bound copies of the *Psychological Index.*[5]

On his next bi-weekly office visit, at which time his abstracts are inspected and criticized, the fieldworker returns his completed cards to the assignment desk with a work record indicating the amount being returned and the balance still in his possession. Returned cards are checked in the record book, and an entry is made on a monthly record sheet from which individual production records are obtained.

e. *Indexing.* Abstracted cards are filed in numerical order by volume number. Cards are assigned by topic to 3 psychologists who, in turn, distribute them to the indexers in their respective groups. The indexers write the indexing formulae (see C, Section IV) and index entries on 5 x 8 sheets, making liberal use of dictionaries, chiefly Warren's, and of a card file of accepted terms, and return the cards and index sheets to the psychologists.

f. *Editing.* The 3 psychologists carefully edit the cards, examining the summary when indicated, checking summary and entries against title, returning a card for a new abstract or for additional information where necessary. The corrected index papers are then discussed with the indexers for the purpose of obtaining, so far as possible, uniform accuracy and maximal consistency of indexers and editors.

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[5] Abstracts have been discovered for 19,000 out of the 40,000 items in Volumes 26 through 35 of the *Psychological Index.* Lists of this material are ready for publication, and those for the remaining volumes will be ready shortly.

g. *Typing.* The indexing formula, from which all index entries are derived, is then typed on the original work card, and each entry is typed on a separate 3 x 5 index card, together with the abbreviated reference. All index cards are proofread twice.

h. *Filing.* Index cards are then filed alphabetically, forming a part of the final index.

IV. INDEXING

The indexing phase has been the most awkward to standardize, largely because of the difficulty of isolating the various aspects of the task. The 2 broadest aspects which, for the purpose of this project, it has been necessary to separate are (1) the formal indexing system to be used and (2) the concrete working-procedure to be followed in its application.

1. The formal indexing system, to be handled mainly by the 4 psychologists, must provide:

a. criteria for the selection of concepts to be indexed;

b. criteria for the selection of words, labels, or terminology for these concepts;

c. a system of cross-referencing (use of *See* and *See also* references) to correct for the scattering which necessarily results from indexing under specific headings.

2. The basic activities involved in indexing a single book or article are shown by job analysis to involve 3 logically separate tasks:

a. selection of concepts, phenomena, or subject matter;

b. selection of words, terminology, or labels for the concepts;

c. arranging and writing these words into index entries.

These operations, being performed by 13 indexers, have been routinized and simplified by (1) analyzing concepts and words into 3 types of " terms " and (2) supplying a notational system for their representation in an " indexing formula," from which all individual entries for a given article may be derived.

This method of indexing by formula[6] has proved workable in the teaching of indexing and has the following advantages: (a) it provides a standard form in which the judgments are to be expressed; (b) it compels consideration of the whole subject matter and organization of the article, thus maintaining relations; (c) it breaks up the complex job of indexing into 2 independent operations: writing the formula (what to index) and writing the entries (how to index it).

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[6] The major work of devising and standardizing the method has been done by R. G. Wetmore. In passing, it may be mentioned that 2 indexers of the *Encyclopedia Britannica,* who appraised the indexing manual, recommended the publication of the " formula method."

Usually the subject matter of a book or article is fairly definite and can be broken down into a limited number of terms in outline form. Three types of terms are distinguished: definitive, limiting, and relational terms.

A. A definitive term represents a concept of primary importance in the article and (often, but not always) in psychology and science. It may be a broad major concept in psychology (learning), a highly specific one (retroactive inhibition), or (less frequently) not a "psychological" term at all, *e.g.* a disease, drug, occupation, organ, animal, religion, proper noun, etc. Thus, in an experimental article the chief ideas which must receive definitive status are (a) the organism studied (if significant), (b) the dependent variable(s), and (c) the independent variable(s). In short, a definitive term may be anything with which the writer was primarily concerned and which represents a proper topic of research, discussion, or study in its own right. It is represented by a capital letter. It is a single concept, but not necessarily a single word. It is always indexed as a heading.

Thus, an article on the intelligence of Navajo Indians contains 2 definitive terms, *Intelligence* and *Navajo,* and the formula is $A+B$. An article comparing Intelligence among Irish, Germans, Danes, and Americans contains 5 definitives, but in accordance with the Class-Member Rule (more than 3 members of a class assume the class name), the last 4 terms are represented by their class term, *Nationality*. The formula is $A+B$, and the definitives are *Intelligence* and *Nationality*.

B. A limiting term is one which, *in the article,* modifies, qualifies, or limits *one* definitive term. It marks off a part, type, application, or aspect of the latter's subject matter. The limiting term is not necessarily an adjective. Usually its significance is dependent on the definitive term. It is represented by a numeral written as a subscript to the letter which it limits, as A_1. It is never indexed as a heading. In the index entry it must necessarily follow its definitive.

The following illustrate A_1 relations:

- Humor
  - subject matter
  - theory
- Learning
  - laws of
- Blood pressure
  - rhythm
  - variability
  - measurement
  - graphic recording

The following types of apparent A_1 relations are actually designated as compound definitives and are rewritten as such or expressed in a single term, in accordance with these 2 criteria: (a) the compound must " hang together " as a single stable concept, not readily decomposable and not a mere phrase; and (b) it must be in common use in the literature:

Intelligence
  defective: ....equals..........Hypophrenia
  mechanical: ....equals..........Mechanical intelligence
  test: ....equals..........Intelligence test
Adjustment
  capacity: ....equals..........Adaptability
  social: ....equals..........Social adjustment
Color
  vision: ....equals..........Color vision
Blood
  pressure..............equals..........Blood pressure

C. A relational term is one which limits or relates to *more than one* definitive term. Examples are correlation, phylogeny, theory, review, and debate. Frequently a relation is understood, implied, or expressed by a particle (in, and, *vs.*), and no relational term is required.

The writing of the formula and, from it, the entries is illustrated below:

Formula	*Entries*	
Crime rate humidity A 1 B	Crime rate humidity	Humidity crime rate
Crime type season A 1 B	Crime type season	Season crime type
Neurosis therapy case A 1 2	Neurosis therapy case	
Eidetic imagery age blind A B C	Eidetic imagery age and blind Age eidetic imagery and blind	Blind eidetic imagery age and

It is understood that no indexing rule or formula is absolute in its application, or universal in its scope, and that judgment can never be entirely eliminated from the task of indexing. Nevertheless, it is believed that the present methodological system will provide for efficient and consistent indexing.

V. Prospects for Completion of the Project

During the course of this project the persons concerned with it have become increasingly aware of the magnitude of the task. No standardized techniques for such work seemed available anywhere which could be adopted wholesale. New and puzzling problems have been and still are being met. An index which shall be near perfect is still the goal. Progress has been slow, and discouragingly so at times. At present the abstracting is somewhat more than 50% complete; the indexing from these abstracts has only recently been started. But the necessary organization of staff and procedure is complete, and the indexing principles have been worked out and have been taught to the workers. There should now be a rapid acceleration of production, barring unforeseen obstacles.[7]

The specific problems of publication are still to be directly faced. Numerous possibilities for printing and methods of financing have been examined. A single volume of a size not too large for convenient use will contain the subject index. A companion volume of equivalent size containing an author index can be readily prepared from the data accumulated for the subject index. Although the latter has not been officially recognized as a part of the program, its publication should be kept in mind.

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[7] There is the ever-present uncertainty concerning the continuance of any W.P.A. project and concerning the continuance of the W.P.A. itself.

SPECIAL REVIEW

WOODWORTH'S EXPERIMENTAL PSYCHOLOGY [1]

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It was a news item of much more than ordinary interest to the psychological world when the announcement was made that "the bible is out," for the word had been awaited for some years not only by the Columbia doctorates but by psychologists everywhere. The thorough competence of its author in almost any field of the subject, as revealed to anyone who has read and heard and interviewed him, coupled with a heavily-ballasted tendency to keep to his middle-of-the-road while also appreciating the views down the by-lanes, and, withal, a simple clarity of exposition, assured an eager public for his *magnum opus*. And now, after several months of examination and use, what is that public's judgment? Does Woodworth's *Experimental psychology* measure up to expectations? Does it reveal competence of grasp, understanding, knowledge? Does it maintain balanced viewpoints? Does it expound complexities simply? It does. Such, at least, is the definite impression received by one member of the psychological public, who has delayed setting down his opinions until he could give the book a tryout as a teaching instrument.

But to take up these points in the reverse of the order of mention—and of importance. First, we have Woodworth's style again. The clarity of his revision of Ladd's *Elements of physiological psychology*, the simplicity of his elementary *Psychology* and his *Care of the body*, and the freshness of treatment of his *Dynamic psychology* are here again. There will be those who feel that he might have stuck closer to certain traditions in verbal usage that make for formal precision. Why, for instance, entitle chapters in an imposing *Experimental psychology* as "The Skin Senses," "Smell and Taste," "Hearing," and "The Sense of Sight," when we have the more formal and scientifically conventional names of the 'cutaneous,'

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[1] R. S. Woodworth. Experimental psychology. New York: Holt, 1938. Pp. xi+889.

'olfactory and gustatory,' 'auditory,' and 'visual'? Still better, why enumerate the psychophysical methods as "the adjustment method," "the method of serial exploration," and "the frequency method" (pp. 395 ff.), when through the years we have known them by other labels—and, particularly, when the very name, 'psychophysical,' has only an historical justification, and the whole field of research is highly formalized.

There are many expressions encountered which are not precisely and stiffly scientific yet are clear enough, and will probably not worry any but the more severe stylist. The following are examples: Müller and Schumann found that alliteration, assonance, or rhyming "made a soft spot in a list" (p. 13); in a blood pressure study "the use of 12 mm as a critical value separated the sheep from the goats" (p. 274); investigators of cold spots "have been on the watch for end bulbs but have not seen any" (p. 463).

The outstanding point about Woodworth's style, however, is that it is readable, and this despite the fact that it is reviewing technical topics, and in a meaty way, so that the reader quickly finds himself immersed and swimming with the current in a stream of factual discussion, all unawares of how he got there. The first-year graduate student no less than the middle-aged experimentalist is likely to find that this book, taken up for an hour's study, will threaten his sleeping program.

Maintenance of a point of view is a consideration of less importance to the author of a work on experimental than to a writer in almost any other psychological line of interest. Yet a curiosity as to coverage and as to organization naturally arises. As will be evident from the chapter titles to be furnished below, the coverage is far more complete than in any other "experimental psychology"—old or recent. No major field is neglected. To this the lone exception is the topic of work and fatigue, as acknowledged by the author, to which, by contrast, Bills, in his *General experimental psychology*, devoted 120 pages, and Robinson, in the "Handbook," gave 80.

The order of topics is not conventional—if there be any such thing—but the placing of chapters on memory and other phases of learning was to be expected by those who had seen the mimeographed pages in use a decade and more ago in the Columbia laboratory. In any case, the matter is of scant importance, for the chapters can be used (as the reviewer has found) in almost any order to fit a particular course of study as already organized.

A fact of major significance is that Woodworth makes very few references to the physiological facts and problems that tie in with many psychological problems, so that his book is far different from the *Handbook of general experimental psychology,* issued by the Clark University Press. In the reviewer's opinion this will have a salutary effect: Many readers and an eminent reviewer of the " Handbook " were so impressed by the detail of material of physiological nature included therein that they felt somewhat apologetic about psychology as a science supposed to stand on its own feet. Experimental psychology, we are now reminded by Woodworth's book, can be psychology and can be experimental without having to lean on physiology. The point is the more impressive when we remember that the author himself came over into psychology from physiology and is known as an author in both sciences.

In its manner of treatment, Woodworth's *Experimental psychology* does not conform to any of the more familiar types. It is not a reference handbook, aiming at completeness of citation and at utter thoroughness of detail of fact (though there are something like 1731 titles in the bibliography). It is not a laboratory manual, describing instrumentation and recommending specific procedures (though the Müller-Urban table of weights is furnished, and broad hints and precautions in laboratory setup are introduced in connection with PGR, chronoscopes, tachistoscopes, and elsewhere). It is not an historical survey (though some historical orientations, like that to reaction time, are strikingly effective). It is none of these kinds of 'experimental psychologies.' It seems designed essentially as a textbook for the advanced student and incidentally as reading for the mature experimentalist. The present reviewer has found it the first adequate answer to the need for a wide range of reading matter to accompany the laboratory training in a graduate experimental course. As said, it is not a manual; and the instructor will have to organize his own course and plan his own experiments and apparatus setups—which is well-nigh the universal practice anyhow. But he can now devote himself to that laboratory work with confidence that the reading materials will take care of themselves.[2]

Our first question is the hardest for a reviewer to attempt to answer. To pass upon the competence of treatment in the many fields that are included in this book is a task that might well be assigned a board of critics who have specialized in their respective lines. The present reviewer will, then, content himself with a series of notes, chapter by chapter, as they present themselves to a psychologist who cannot go back into the references to verify all restatements and interpretations nor always evaluate competently the author's choice of materials.

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[2] Attention should be directed to G. R. Wendt's monograph (Methods of recording action. *Arch. Psychol., N. Y.,* 1938, No. 228), originally planned as an appendix to the Woodworth book, in which principles of apparatus construction and employment are set forth.

I. *Introduction.* The reader is introduced to the experimental method as one that emphasizes controls that affect independent and dependent variables. The interdependence of experimental and statistical procedures is pointed out; and a Fisher reference is given, though not his *Design of experiments.*

II. *Memory;* III. *Retention;* IV. *Memory for Form.* The general topic of learning is allowed to fall apart into the historically distinct topics of laboratory investigation in separate chapters. In Chapter II a few more suggestions on apparatus would have been helpful, for surely one of the hardest problems for the general laboratory is that of providing memory apparatus that is sure-fire, simple, and rugged; and all the ingenuities of series-building, time-spacing, and the like come to naught when an exposure apparatus skips a beat or fails one. Ebbinghaus is, of course, the point of departure, and the range of problems and methods is fairly adequately covered. Eidetic images are included, in a factual treatment rather than a theoretical exposition, beyond the statement that the eidetic child "sees objects rather than masses of light, shade, and color" (p. 46)—which is peculiarly in line with some other Woodworthian emphases to be mentioned.

The wide range of material on retention (from which only Luh's differentiation of measurement methods was seriously missed) is so assembled without forcing as to throw doubt on the theory of a physiological consolidation process. This is shown especially in the moot question of reminiscence. It is a bit startling to read that the relations of memorizing, retention, recall, and relearning may be likened to the relations that can be traced in the response of the muscle to exercise and rest (p. 60), and a bit dangerous, too, as we know from the adoption of the analogy by the ancient 'formal discipline' (p. 177).

In Chapter IV is an excellent example of studies from a wide range, all presented for their own value, yet also, without forcing, serving as evidences pro and con on a quite fundamental problem: Wulf's theory that the memory trace tends toward "better figure." The reader, then, is afforded a good review of the more significant research done, is acquainted with important theory, and has a viewpoint mildly suggested. Incidentally, here is Woodworth's characteristic sympathy and grasp of a new viewpoint, without his becoming stampeded by it to the left or bending backward to the right.

V. *The Conditioned Response.* This chapter is comparable with Hull's chapter in the "Handbook," for, though not quite so complete in citations (some 70 references as against 119), it is as satisfactory for orientation and for analyses of the problems, especially 4 years later. Of note is the recognition of differences, recently come home to us, between 3 sorts of conditioning experiments as represented by those of Pavlov and Liddell (no avoidance of US), of Culler and associates (avoidance of US by the CR), and of Skinner (CR produces US) (pp. 102 ff.). A good point on the development of drowsiness appears in the statement that each trial, on which the animal is fed only a small amount, really involves a premature cessation of feeding. A lesser-known method of inducing neurosis appears in the experimental disruption of a salivary CR by building up too strong a shock. No mention is made of Steckle and Renshaw's experiment contradicting Hudgins' conclusion: Is that question settled? A ventilation and broadening of the concept of CR incidentally results from the manner of treatment, and comes out clearly in emphases upon attentional and attitudinal factors (pp. 99 f., 109 ff.) and upon the conditioned withdrawal as not a particular movement so much as a withdrawal from a particular object (p. 123).

VI. *Maze Learning.* Why this should be included and not discrimination learning—both coming from the animal laboratory and both most fruitful there—is not at once apparent. True, the rat-in-the-maze furnishes the most used analogy in theorizing on learning; yet the discrimination habit is coming into its own in work upon equivalence of stimuli and sense-perceptual problems. A refreshing side to this chapter is Woodworth's hypothesis of place learning, which shifts emphasis from the motor components of an activity to the objects or situation with which it deals, found also in his treatment of the CR in the preceding chapter. One wonders, however, if he is not dealing with a straw man in his handling of the 'motor pattern theory' which he identifies with the Watsonian chain reflex: Must a motor pattern be an invariable one?

VII. *Practice and Skill;* VIII. *Transfer of Training;* IX. *Economy and Interference in Learning and Remembering.* Neither in preceding chapters nor here do we run into the law of effect. As has been the case with most of the other explanatory principles of selection and fixation that used to grace our texts on learning, has this one, too, been weighed and found wanting? It must be that he has misgivings about the trial-and-error doctrine itself, for it gets no mention until a very late chapter in the book (XXIX), where his survey of animal experiments fails "to reveal any case where either insight or trial and error was present" in its extreme form (p. 762). Omission of the law of effect may be associated with Woodworth's acknowledged omission of research on motivation (for the reviewer believes that recent work is linking the two closely). To be sure, nearly all the clean-cut work on motivation as a factor in learning has been conducted in the animal field, but the findings are highly suggestive for human psychology.

Cross-education is included (starting with Weber!), though some would question its strict relevance to the problems as usually conceived. Swift's curves for ball-tossing are reproduced without Peterson's correction thereof, but the omission may be justified since they are used as evidence for cross-education, not for positive acceleration. The Poffenberger-Wylie-Bruce interpretation is happily included. As for certain distinctions that have been magnified in the literature: 'abstract' principles are, after all, concrete bits of behavior (p. 207).

Chapter IX is limited to recitation, massed *vs.* spaced, whole *vs.* part, interference, and retroactive inhibition. Studies on the last-named are reviewed as favoring either the interference theory or the anticonsolidation theory.

X. *Feeling.* This is a short chapter on introspective analyses, with particular attention to Nafe's characterizations of bright and dull pressure. Woodworth "wonders whether Nafe's original experiment would give the same result in other laboratories" (p. 238) and offers the conclusion, though "not exactly forced by the evidence," that feelings are reactive attitudes (p. 241). This chapter will surely strike conservatives as a bit too summary; and for the many subtle ramifications of feeling problems and their interpenetration with other phenomena one will want to keep Beebe-Center's book at hand.

XI. *Expression of the Emotions;* XII. *Bodily Changes in Emotion;* XIII. *The "Psychogalvanic Reflex" or "Galvanic Skin Response."* "So far definite, dependable symptoms of feelings have not been discovered. From the original point of view the method of expression has broken down" (p. 235). This statement in the preceding chapter apparently concerns the elementary feelings only; for in this and the 2 following chapters methods of expression are given generous space (12 pages on facial expression). Vibrato makes its entrance here instead of in connection with esthetics later, but which way the Iowa laboratory would have it may be debatable.

"Statistically, the I/E ratio is not a very good measure. It is all right for single cases but not good for averaging, when the single ratios differ considerably. . . The I-fraction [here recommended] is obtained by dividing the duration of inspiration by the duration of the whole cycle" (p. 262). True, the I/E ratio has no upper limit, while the I-fraction has the virtue of varying only between .00 and 1.00; but the skewed character of breathing-cycle curves should render the latter inadequate also. Simple physiological and apparatus notes are furnished to orient the student regarding respiratory rate and amplitude, the I/E ratio, and circulatory volume, rate, and pressure; but there is scant attention to the problem of motor patterns. The history of attempts, from Féré and Wundt down to the present, has indeed furnished a procession of failures; but at least our one apparent achievement, the startle pattern, might be proudly displayed.

Woodworth's preference for 'psychogalvanic reflex' over the 'galvanic skin response' is likely to be frowned upon; but if we are to have more modern terminology why not go the whole way of Ruckmick's "electrodermal response"? The "rudiments of the necessary technique" do not include mention of the tachogram, electronic methods, or reflexohmmeters. Space is given to a physiological discussion of 6 pages (probably the longest in the book), that does not overbalance 13 pages of psychological queries.

XIV. *Reaction Time.* Here is one of the best examples of historical orientation, done apparently casually but putting the reader abreast of all the significant questions; and the whole chapter is Woodworth at his best.

XV. *Association.* Following XIV, the topic of association is treated quantitatively as well as qualitatively and clinically.

XVI. *Experimental Esthetics.* After a description of the methods of impression procedures available, results on color and visible form are set forth.

XVII. *The Psychophysical Methods;* XVIII. *Results in Psychophysics: Judgments of Magnitude.* The only topic in the book that is presented too abruptly is that of the methods of psychophysics, where, if anywhere, deliberateness is to be sought. The queries concerning this and that variation in procedure are rather inclusively brought to the reader's attention; but in some ways the treatment is likely not to be as successful as, for example, Guilford's, in his *Psychometric methods*. The 'reaction time method in psychophysics' is included—at the expense of the more classical methods of equivalents and of equal sense-distances.

XIX. *The Skin Senses;* XX. *Smell and Taste;* XXI. *Hearing.* Woodworth's freedom from too much bias by his reaction conception of psychology is revealed by the dominant place given here to introspective analyses and classifications. Indeed, Henning's work gets nearly 10 pages of pros and cons, an overgenerous amount; and adequate presentations of analyses by Stevens, Rich, Köhler, and others crowd descriptions of the physical-physiological work of Wever and Bray, Fletcher, Kreezer, Davis, and others. Is localization of sound a sensory or a perceptual problem? At least it is awarded 16 pages in this context. The term "quality difference" when referring to physical stimuli is likely to be misleading (p. 521) and is probably a slip. The meaning of "decibel" is not made too clear (pp. 437, 505).

XXII. *The Sense of Sight.* Again the phenomenal aspects of sensation are given major treatment, starting from Katz's varieties and including 6 pages on after-images and their explanations. But the stimulus correlates of many sorts of visual phenomena are not neglected. Woodworth is not invariably happy in his substitution of new names for old, but 'duplex' for 'duplicity' in translating Von Kries's *Duplizitätstheorie* seems eminently sensible.

XXIV. *Perception of Color;* XXV. *The Perception of Form;* XXVI. *Visual Space.* Problems of color constancy, especially the modifications of illumination factors, receive a 20-page treatment, through which Woodworth's (James's) view of perceiving as a perceiving of objects emerges at times, but unobtrusively. Unobtrusively again, it appears in the canvass of the leading problems in form perception, which are set up each in its own right and not as the product of the Gestalt or any other school. The geometrical illusions, stereoscopy, and distance cues appear in their conventional way, with stress laid upon the visual as against tactile-kinesthetic cues. The phi-phenomenon is not exploited beyond 2 condensed paragraphs, for some reason.

XXIII. *Eye Movements;* XXVIII. *Reading.* Two models of presentation that tie in together.

XXVII. *Attention.* "Besides this inclination [on the part of Titchenerian, Gestalt, and other psychologists] to place a taboo on the concept of attention, there is an obvious difficulty in segregating certain experiments under this title. . . Yet these experiments have distinctive characteristics which ought not to be obscured" (p. 684): span of apprehension, shifts, fluctuations, distractions, simultaneous performances.

XXIX. *Problem Solving Behavior;* XXX. *Thinking.* Except for Chapter V and some of Chapter VI, Chapter XXIX contains the only considerable inclusion of research work on animals and children, due to the fact that that is where the fundamental concepts have largely arisen or at least have been experimentally formulated and tried out. History is incidentally set straight with a query as to "whether or not this motor conception of trial and error is precisely what Thorndike meant" in his original monograph (p. 752), and with Hobhouse credited with origination of the methods of string-pulling, reaching-stick, 2 sticks, footstool, and others of the sort. "Köhler's criteria [for insight] of suddenness and smoothness" are, in fact, not always maintained by him, as Woodworth would seem to imply (p. 761). Gibson and McGarvey had said in their review of experimental studies of thought and reasoning that "less ingenuity in inventing methods seems to have been exerted in this field [of thinking] than in almost any other" (*Psychol. Bull.,* 1937, **34**, 344). The judgment still seems borne out by the survey Woodworth gives us, for after the 'free report' studies 2 decades ago by the Würzburg School, Binet, Bühler, and himself, Selz's is almost the sole contribution, if we except those on concept-forming and other forms of induction. Particularly is there a crying need for experimental attack upon the creative processes, which get recognition here in their case-method form. However, this impression of a paucity of laboratory work which the reader gets from Woodworth's final chapter may be exaggerated by the scattering of some relevant studies in other chapters, and by the omission of references to research on the ultimate nature of 'silent thinking' or 'inner speech' through the avenues of action-current and other muscular recordings—which is apparently too physiological for inclusion.

Through this running account the reviewer has tried to imply that Woodworth's *Experimental psychology* is astonishingly complete in its coverage—despite his having had to withdraw a substantial amount of his completed manuscript; that it is remarkably well balanced in its distribution of attention to all the topics of the conventional field of 'experimental psychology'; that it is as unbiased and fair a review as seems humanly possible; that he who seeks the author's own interpretative viewpoints can find them suggested here and there quite unobtrusively but forming a consistent pattern. For the psychologist's reading shelf—or better, his desk—it is item No. 1. For the teacher of a course in 'experimental' it is an answer to prayer.

The publishers are to be congratulated on a first-class bookmaking job. The absence of the titles to journal articles listed in the bibliography is somewhat tantalizing, and could have been gotten into much the same space by a reduction in type size for that section. Italics for statistical symbols, and italics or quotes for words and syllables referred to as such in the text, would aid the reader. Publishers' conventional setting of figures at the middle of pages sometimes throws them out of place with the text. But these are insignificant! More important are the large, clear type on excellent paper, generous employment of center and side heads, clear figures, and a binding that is rugged, handsome, and moisture-resistant.

BOOKS RECEIVED

Gilliland, A. R., & Clark, E. L. Psychology of individual differences. New York: Prentice-Hall, 1939. Pp. xvi+535.

Goulden, C. H. Methods of statistical analysis. New York: Wiley, 1939. Pp. vii+277.

Jaloux, C. E., *et al.* Le mystère animal. ("Présences.") Paris: Plon, 8, Rue Garancière, VI^e, 1939. Pp. ix+301.

Netzer, R. F. The evaluation of a technique for measuring improvement in oral composition. *Univ. Ia Stud. Educ.*, Vol. X, No. 4. Iowa City: University, 1939. Pp. 48.

Pratt, C. C. The logic of modern psychology. New York: Macmillan, 1939. Pp. xvi+185.

SPECIAL NOTICE

After June 20 all manuscripts, books for review, and correspondence concerning the *Psychological Bulletin* should be sent to the editor, Professor John A. McGeoch, at the Department of Psychology, The State University of Iowa, Iowa City, Iowa.

NOTES AND NEWS

At the meeting of the Society of Experimental Psychologists in Princeton, April 3–4, the fourth award of the Warren Medal was made to Dr. Carlyle F. Jacobsen for his "Description of the Function of the Frontal Lobes of the Brain."

Dr. Frank N. Freeman, professor of educational psychology at the University of Chicago, has been appointed chairman of the department of psychology. Concurrently with the appointment, the department has been transferred from the Division of Biological Sciences to the Division of Social Sciences. Professor Freeman succeeds Professor Harvey Carr, who retired last autumn. In the interval, Professor Louis L. Thurstone, Charles F. Grey distinguished service professor of psychology, assumed responsibility for the administrative details of the department pending the appointment of a chairman. The transfer of the department to the Division of Social Sciences for administrative purposes was made because Dr. Freeman is connected with that division. The field of interest of the department will continue to be in both biological sciences and social sciences.—*Science*.

Dr. Norman R. F. Maier, assistant professor of psychology at the University of Michigan, who received the award of $1,000 of the American Association for the Advancement of Science at the Richmond meeting, has received from the university the Henry Russel Award for 1938–39 in recognition of his scientific work. The Russel award and the Henry Russel lectureship were made possible by a bequest of the late Henry Russel, of Detroit, an alumnus of the University of Michigan. —*Science*.

Dr. R. S. Sackett, who has been assistant professor of psychology at the American University, has accepted an appointment as research associate with the Educational Motion Picture Project of the American Council on Education, Washington, D. C.

The Society for the Psychological Study of Social Issues has been granted $1,000 by the William C. Whitney Foundation for the support of its research activities during 1939.

The Fifth International Congress for the Unity of Science will be held at Harvard University, September 3–9, 1939. A number of general sessions will be held on such themes as "The Logical Unity of Science," "Cosmology and Physics," "The Relation of Biology and Psychology," and "The Place of the Social Sciences and Humanistic Sciences in the Edifice of Science." Information may be obtained from the American Congress Headquarters, Department of Philosophy, University of Chicago.

THE Washington-Baltimore branch of the American Psychological Association held its third meeting of the year at the George Washington University, Washington, D. C., on March 15. The following program was presented:

MITCHELL DREESE: "A Comparison of Methods of Administering and Scoring the True-False Test."

THELMA HUNT: "Psychological Studies of Patients Undergoing Frontal Lobotomies."

MADISON BENTLEY: "Wish, Wisdom, and Experiment in Psychology."

A NEW psychological laboratory has been opened at the University of Alabama, as part of the recently established department of psychology in the College of Arts and Sciences, which first offered courses in psychology in September, 1937, under the direction of the present chairman, Professor Donald A. Ramsdell. The stock of apparatus includes a necessary nucleus for courses in experimental and physiological psychology, demonstrations in elementary psychology, as well as equipment for a limited amount of independent experimentation. Dr. John T. Cowles has immediate charge of this laboratory. Visiting psychologists are welcomed cordially.

THE *Journal of Consulting Psychology* announces that the entire set of photographs of applied psychologists which appeared in Volumes I and II of that journal is now available. These photographs, reproduced on 100-pound gloss-finish paper, are 7½ by 10 inches and are suitable for framing or, if desired, they may be tacked directly to the wall.

The psychologists whose pictures are in this series are: W. V. Bingham, J. McKeen Cattell, Douglas Fryer, Henry H. Goddard, Leta S. Hollingworth, A. T. Poffenberger, Lewis M. Terman, E. L. Thorndike, L. L. Thurstone, Lightner Witmer, Robert S. Woodworth, and Robert M. Yerkes.

These photographs will be sent postpaid on receipt of $1.50. Single copies are 50¢ each. Checks should be made payable to Jack W. Dunlap, Treasurer, A. A. A. P., and sent to the University of Rochester, Rochester, N. Y.

THE summer meeting of the Rocky Mountain branch of the American Psychological Association will be held at Boulder, Colorado, about the middle of July. Members and Associates of the American Psychological Association who will be in the Rocky Mountain area during that time are invited to contribute papers and participate in the meetings. Titles and abstracts of papers should be sent to Dr. George T. Avery, at Fort Collins, Colorado, by June 15.

THE International Committee has voted to hold the next meeting of the International Congress of Psychology in Edinburgh in the summer of 1940.

Vol. 36, No. 7 July, 1939

Psychological Bulletin

Forty-seventh Annual Meeting
of the
American Psychological Association

Stanford University
and the
University of California

Monday, Tuesday, Wednesday, and Thursday
September 4, 5, 6, 7
1939

Officers of the Association

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The President, Secretary, Warner Brown, and Calvin P. Stone

Program Committee

Elmer K. Culler, Edward B. Greene, and the Secretary

Please bring this program to the meeting. Additional copies will be sold at the Registration Desk for 25 cents each and will be obtainable later from the Business Manager.

CONDENSED PROGRAM SCHEDULE

Registration and Informal Gatherings at Headquarters in the Stanford Union will be held throughout the meeting.

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PROGRAM

GENERAL

Monday, September 4, 9:00 A.M.
Room 2, Education Building
WALTER R. MILES, Chairman

9:00 A.M. *Evidence for the Recording Error Criticism of ESP Data.* JOHN L. KENNEDY, Stanford University.

Photostatic copies of original records from J. D. MacFarland's experiment, "Discrimination Shown Between Experimenters by Subjects" (*J. Parapsychol.*, 1938, **2**, 160–170), were reëxamined for recording errors. This experiment deviates from the usual ESP recording methods in that two experimenters made independent records of the subjects' calls. A total of 69 errors which affected the score was found, only 24 of which were discovered by the two independent checks discussed in the experimental report.

Evidence for a large number of recording errors of the "inversion" type in the card column was obtained by an analysis based upon the extra-chance occurrence of adjacent pairs of hits. When subtractions indicated by this analysis are made, the extra-chance scores reported for MacFarland's experiment are reduced to the chance level. [10 min., slides.]

9:15 A.M. *A Theoretical Consideration of the Basis for Approach-Withdrawal Adjustments in Behavior.* T. C. SCHNEIRLA, Washington Square College, New York University.

Various theoretical systems in psychology have been built upon *a priori* acceptance of a negative-positive differentiation in behavior. However, the conditions which are responsible for the ontogenetic appearance of such a differentiation appear to be unclearly glimpsed.

A consideration of the evidence for infra-mammalian animals shows a more or less distinctly inherent basis for a "sign" differentiation of behavior. This rests upon the properties of stimulation at different intensities in accounting not only for quantitative differences, but especially for qualitative differences in adjustment to the environment. An analysis of adaptive behavior in one well-investigated animal shows that stimuli at high intensities typically elicit a general reaction in which the predominance of a differentiated excitation-reaction system virtually insures withdrawal from the source. Conversely, weak stimuli predominantly arouse the antagonistic system and typically produce an approach to the source of stimulation.

This conception envisages negative-positive responses as fundamentally attributable to the differential arousal of excitation-reaction systems which function as though they possessed distinctively different activation thresholds. Through natural selection, we may hypothecate the evolution of such mechanisms along adaptive lines.

This view has utility as a hypothetical basis for understanding approach-withdrawal behavior in man. The problem has been dealt with previously in a rather fragmentary way, evidently because of complexity and the initial absence of directionalized general reactions in neonate behavior. Evidence from general studies of early behavior, of emotional development, and of certain crucial adjustments such as the reaching reaction, may be profitably organized on this basis. The bearing of this "reaction-system-threshold" theory upon concepts such as "appetite-aversion," "vectors," "sign-gestalt" and the like, will be considered. [15 min., slides.]

9:35 A.M. *Some Critical Revisions and Extensions of Lewin's Topological and Hodological Psychology.* Robert Leeper, University of Oregon.

The publication in December, 1938, of Lewin's monograph, on directions of forces within the life space and on methods of measurement of psychological forces, rounds out his discussion of basic constructs for the psychology of motivation, and makes opportune a critical discussion of his system as a whole.

Certain confusions and misinterpretations are found within Lewin's treatment because (1) he defines regions within the life space as indicating sometimes anticipated activities, and sometimes *objects* of activity, (2) he uses the concept of "barrier" to refer sometimes to psychological facts (regions within the life space) and sometimes to objective obstacles whether or not they are appreciated as such by the person, (3) he speaks of the life space as permitting locomotions through it, whereas locomotions, except as anticipated, seem always to be objective activities. The implications are pointed out that follow from stricter adherence to Lewin's own best usages.

Some extensions are needed, especially to recognize several basic types of conflict situation, one of which results from anticipated irreversible paths, and another from forces resulting from anticipated locomotions of the surrounding field. Some situations require a concept of "decision-serving screens."

Lewin's definitions of direction and of relationships between directions are found inapplicable to a number of important situations, and in some cases are found not to conform with such criteria as summation of forces, creation of tension states, etc., which might indicate equality, oppositeness, and partial equality of direction. New definitions of direction and of relationships between directions are proposed that attempt to conform more adequately to Lewin's basic dictum that direction is a property of paths as wholes. [15 min.]

9:55 A.M. *The Unit of Description in Psychology.* KARL F. MUENZINGER, University of Colorado.

In order to make a scientific analysis of behavior it is necessary to have a unit of description. Any scientific analysis attempts to reduce the chaotic complexity of observed events to a systematic simplicity of lawful relationships which transcend the geographic-historical settings of the events. A unit of description of psychological events makes it possible to give a simplified and systematic description of the stream of behavior, and to compare different segments of this stream with each other in order to discover recurring similarities and propose lawful relationships. Such a unit has the further advantage that it provides a common theoretical framework for diverse problems and experiments.

Three different units are described and evaluated: (1) stimulus-response; (2) the psychological field; and (3) the start-to-end-phase unit comprising a cycle during which a single direction of behavior can be observed. It is claimed that the third unit offers a more adequate basis for a systematic analysis of behavior than do the other two. Within the start-to-end-phase unit we can distinguish four types of factors which permit a complete description of behavior. The factors of motivation determine the direction of behavior and the strength with which it moves towards the end-phase. The factors of discrimination break up the psychological situation into elements (that is, things and what they do) which guide behavior towards the end-phase. The factors of performance modify the situation (by locomotion, manipulation, or speech) so that the end-phase will be brought about. And finally, any changes in the dynamic relationships within the psychological situation (as by blocking, removal of obstacles, or threat of danger) generate affective factors. [15 min., slides.]

10:15 A.M. *Psychological Distance as a Function of the Intensity of Psychological Need.* RICHARD S. CRUTCHFIELD, Mount Holyoke College.

A series of experiments were undertaken to test the hypothesis that for the rat the magnitude of psychological distance in the psychological field is directly influenced by intensity of need.

Twenty blind rats partially learned the problem of making a turn to food always in side-path number six of a series of ten similar non-food side-paths spaced equally along an eleven-foot elevated runway. Sensory cues were controlled, so that the relatively high level of performance finally achieved by the rats is explicable only in terms of 'kinesthesis,' broadly defined, or preferably, in terms of an hypothesis of preparatory set or expectancy, *i.e.*, 'behavior potential,' of the animal for expenditure of a specific amount of work in running the required distance.

Critical trials were given in which various groups had an intensity of hunger drive either increased or decreased from the normal level. The results of these critical trials indicate that under conditions of increased drive the rats run a greater average distance before making their turn, and under conditions of decreased drive they run a lesser average distance before the turn, as compared with the distances on control trials under normal intensity of drive.

Similar experiments with blind rats on a 10-unit linear maze, having one unique left turn in a series of right turns, indicate that the magnitude of the psychological distance to the unique turn, which is an outstanding feature of the cognitive structure of this maze, is dependent upon the intensity of drive. In general, then, it is indicated that the dimension of distance in the psychological field is variable with respect to intensity of psychological need.

Further discussion and interpretations are given. [15 min., slides.]

10:35 A.M. *Interregional Migration of Psychological Talent.* J. E. GREENE, University of Georgia.

Scope of the Study.—As herein used, "psychological talent" refers to the 1,218 psychologists listed in the 6th edition (1938) of *American Men of Science*. Making use of Odum's classification of the United States into seven regions, data were obtained on each of the following six categories of interregional migration: (1) *nativity—training* interregional migrations; (2) *nativity—first employment* interregional migrations; (3) *nativity—present employment* interregional migrations; (4) *training—first employment* interregional migrations; (5) *training—present employment* interregional migrations; (6) *first employment—present employment* interregional migrations.

Procedure.—The job analysis involved includes the following procedures: (1) Editing of biographical sketches in the 6th edition (1938) of *American Men of Science* to isolate psychologists. (2) Transcribing of selected data from the edited biographical sketches to transcription sheets. (3) Coding of transcribed items. (4) Punching of coded items in individual Hollerith cards. (5) Sorting of punched cards to secure desired classification of data. (6) Analysis of data (including the calculation of coefficients of contingency; the amount and reliability of differences in percentage of interregional migration, of each of the six types, at different levels of training; etc.).

Results.—The above-described data are analyzed in terms of the six major categories of interregional migrations and in the light of available supplementary data. The various coefficients of contingency indicate that positive interrelationships exist among each of the four variables (regions of *nativity, training; first employment, present employment*), at each of the three levels of training (bachelor, master, doctorate). However, the extent of the relationship varies notably from one migration type to another and from one level of training to another. Marked and significant regional differences in the six types of interregoinal migration are analyzed and their implications discussed. (Acknowledgment of aid by Mr. W. F. Lawrence, a non-member graduate fellow, is gratefully made.) [15 min.]

10:55 A.M. *What Is Special Ability?* NORMAN C. MEIER, University of Iowa.

A new conception of special ability is offered, disclosing a curious interlinkage of hereditary elements (ancestral occupations) with hereditarily conditioned development. A theory of 'constitutional stock' inheritance is advanced as the most logical interpretation of accumulated data. This report is a concluding statement of the Spelman-Carnegie Art Research Project at Iowa and refers to work of twenty assistants and the participation of thousands of children and adult subjects over a ten-year period.

As now understood, a special ability is a combination of factors—some hereditary, some acquired on a conditioned basis—that drive toward and facilitate accomplishment in the field. In the instance of artistic aptitude (the more inclusive term) six fundamental factors were found normally to be present. Three have reference primarily to heredity (setting limits of development), identified as manual *skill* or craftsman ability, volitional *perseveration,* and aesthetic *intelligence.* While the other three, isolated as *perceptual* facility, creative *imagination,* and aesthetic *judgment,* are regarded as primarily acquired, they are to some extent conditioned in their development by special (stock) inheritance. Some degree of constitutional stock inheritance is hence indispensable, since it not only provides the psycho-somatic conditions for the ready acquisition of craftsman skills but also conditions in the artist the functioning of perceptual, imaginative and judgment processes. The craftsman habit of work thus developed tends to direct a particular manner of perceiving and sets the framework for the judgment process. The artist thus illustrates a curious interlinkage of hereditary elements (ancestral occupations) and present particular modes of learning. The finical regard of the disciplined artisan for nice order and proper placement is foundational to a later studied regard for the demands of good balance, rhythm and proportion. Other factors will be discussed. [15 min., slides.]

CLINICAL

Monday, September 4, 9:00 A.M.
Room 521, Hydraulic Laboratory
ROBERT G. BERNREUTER, Chairman

9:00 A.M. *Psychoanalysis and Experimental Methods.* BELA MITTELMANN and HAROLD G. WOLFF, New York Hospital and Department of Medicine, Cornell University Medical College.

The correlation of psychoanalytic and physiological data resulted in an increase in accuracy in method, concept and formulation. The finger temperature of experimental subjects was recorded during affective stress induced by prolonged discussion of distressing life situations. Secondly, the finger temperature was continuously recorded during regular interviews of patients under psychoanalytic investigation. The patient's affective state at any given moment during the analytic hour was ascertained and correlated with the simultaneously observed physiological change.

Observations may be formulated as follows: (1) Emotional reactions were complex with one affect dominating at the moment. (2) Predominant fear, anger, depression, elation were accompanied by a fall in finger temperature. (3) During emotional reactions of which the subject was unaware, yet which as judged by speech and behavior were apparently existent, the finger temperature also fell. (4) During attitudes of "detachment" toward disturbing situations with illusory attitudes of security and self-esteem, the finger temperature remained at the relaxation level. (5) Major sustained falls in finger temperature were associated with states of tension, anxiety and conflict. (Maximum fall observed was 13.5° C.) (6) During periods of sexual phantasy the finger temperature rose. (7) Thus, it was shown in detail how certain personality needs manifested themselves in significant situations, and how the emotional reactions and phantasies were correlated with the appearance and disappearance of physiological changes. [15 min., slides.]

9:20 A.M. *An Association-Set Method in Psychotherapy.* DOROTHY HAZELTINE YATES, San José State College.

The problem was to find a quick and lasting method of dispelling undesirable emotional reactions. The method found most effective is best described by example. A girl of nineteen was much troubled by her habit of bursting into tears when with men, especially her father. The remedy lay in building strong associations of relaxation, calmness, and security, with a word regarded by the subject as particularly meaningful, in this case, the word "calm." While in a very relaxed state, the subject was instructed to think of the word "calm" and associate with it certain definite ideas of peace, security, and well-being, always with the conviction that at any future time the word "calm" would reinstate the same condition of physical and mental relaxation and composure. There were no failures by this method. Young adults were cured—no relapses after two years—after three one-hour treatments. [10 min.]

9:35 A.M. *An Experimental Study of the Process of Adjustment in Delinquent and Nondelinquent Children.* H. MAX HOUTCHENS, Division for Children, Washington State Department of Social Security, Olympia, Wash.

This is the report on one of a series of performances the experimenter is attempting to standardize for prognostic purposes in the field of child adjustment. It is the purpose of the study to substantiate experimentally that there are measurable determining tendencies that develop in, or are causative of, a delinquent population.

In this experiment the subject's abilities to break down old perceptual patterns and replace them with new patterns obtained in mirror-tracing acts are observed. Significant differences are obtained in both qualitative and quantitative comparison of delinquent and nondelinquent children.

Kinetic sets appear to have a close relationship to adjustment patterns. [10 min., slides.]

9:50 A.M. *An Approach to the Mental Organization of the Feeble-Minded.* ALICE J. PHILP, Letchworth Village and Institute of Child Welfare, University of Minnesota.

The statistical definition of feeble-mindedness as the lower end of the distribution curve of Binet "intelligence" is rather widely held. Mental deficiency so considered is but a small amount of the composite of abilities possessed by normals. Lewin suggests, however, that the entire mental make-up of the feeble-minded individual differs qualitatively from that of the average individual, is rigid, and lacks fluidity.

To study this question, two groups of 25 boys each, matched for Binet mental age (7–9), all inmates of Letchworth Village, were administered individually the following five tests: Rorschach, scored with reference to "constriction" *versus* "dilation" of the total personality; variations of Weigl's tests measuring ability to change one's set spontaneously; a simplification of the Vigotsky concept-formation test; a modification of Maller's perseveration test; and a peg-board position learning test measuring interference and transfer of training.

The younger boys had an average chronological age of 12 and an average I.Q. of 70, the older group, an average C.A. of 20, and I.Q. of 50. The higher group fell into the "borderline" classification of intelligence and were primarily behavior problems before commitment; while the older group were, without question, mentally incapable of existing independently. No case of feeble-mindedness as the result of any known injury was included.

Test results will be discussed with reference to differences in C.A., I.Q., Binet pattern, and institutional adjustment in an attempt to evaluate Lewin's theory of feeble-mindedness and other concepts of general intelligence. [15 min., slides.]

10:10 A.M. *Differential Syndromes in Stanford Binet Failures.* FLORENCE MATEER, Merryheart Clinic, Columbus, Ohio.

Progress in clinical psychology depends upon its ability to ameliorate the condition of the individual who seeks psychological help.

Relief, or corrective procedure, depends upon proper clinical identification of the handicapping symptoms.

Differential syndromes of symptoms for hypothyroidism and hypopituitarism, at least, present themselves in the relative failures of such cases on the Stanford Binet and other test series. For instance, inferiority in the memory span for the repetition of digits marks the hypothyroid child in even mild cases.

Such syndromes vary for different causal factors yet are highly similar in cases affected by the same handicapping factor regardless of actual mental level.

Treatment modifies and often totally eliminates the syndrome, thus rapidly increasing the mental age. It may reappear with premature cessation of medication, the mental age dropping or remaining static.

This is a field which can be handled only by full coöperation with the medical sciences. [10 min.]

10:25 A.M. *A Statistical Analysis of the Validity of the Babcock Test of Mental Deterioration.* AGNES A. SHARP, The Psychiatric Institute of the Municipal Court of Chicago.

A practical problem in clinical psychology is the determination of the degree of mental deterioration in humans which should be termed pathologic and which indicates need for treatment of the patient in a mental hospital.

Quantitative tests of impairment of mental functioning are difficult to devise. Harriet Babcock made such a scale, standardized it on a paretic group, and published her results as "An Experiment in the Measurement of Mental Deterioration." Her " method of determining the degree of mental deterioration was to measure the discrepancy between scores on (Terman) vocabulary test which requires only previously formed associations and correlates highly with intellectual level, and scores on other tests which emphasize new learning and speed, that is, mental efficiency." Her other tests include digits, pronunciation, immediate reproduction, maze, substitution, Knox Cube, etc.

The immediate concern of this study has been to determine the validity of the Babcock Test when used with chronic alcoholics and other non-paretic patients. To that end the Babcock Test scores of 81 patients, whom the psychiatrists in the Psychiatric Institute qualitatively diagnosed as markedly deteriorated mentally, were analyzed statistically. The analyses made were: (1) The reliability of the short form of 20 tests used in this study, as determined by the Spearman-Brown formula, was .899. The longer form of 30 tests yielded a reliability of .931 by the same formula. (2) The r between the vocabulary test and each test was determined. The range was found to be from .083 to .755. (3) An item analysis based on inter-test correlations. (4) A factorial analysis was made to determine the factors present. We chose five factors which we have tentatively identified as (a) verbal, (b) motor, (c) memory, (d) an ability and/or willingness to manipulate abstractions, (e) mental fluency. [15 min., slides.]

10:45 A.M. *A Study of Specific Traits in Cases of Alexia.* GRACE M. FERNALD, University of California.

This paper reports findings based on a study of 60 cases of alexia and a study of autobiographies and biographies of individuals having similar disabilities.

The cases included in the experimental study are all individuals of normal intelligence, ranging in age from nine to twenty-two years. Results are given (1) of a study of the emotional adjustments in each case, (2) of detailed records of the learning process, (3) of certain experiments in learning Braille, (4) of the analysis of mental tests.

Results. (1) In all cases we found emotional instability a characteristic of the individual. In all but four cases the emotional maladjustment began only after or at the same time as the failure to learn to read. In all cases the emotional stability was restored by the development of normal skill in reading and other subjects without attention to the emotional problem as such. (2) In all cases the learning curve for reading was normal when the technique was adapted to the individual. Certain peculiarities were found in the learning process such as the use of kinesthetic methods and a blocking of the learning progress when the use of these methods was interfered with or auditory distractions were introduced. In all cases the individual responded slowly to verbal directions. (3) In learning Braille, the alexia cases slightly surpassed the control group. In all cases the subject could read Braille with only the visual stimulus, after it had been learned in the usual way, even in cases in which he was still unable to read the printed word. (4) A study of the results of intelligence and other tests show specific respects with reference to which the alexia cases differ from the average normal individual.

The descriptions found in autobiographies and biographies of individuals who report difficulty in reading and spelling agree with the results of our experimental studies. [15 min.]

BRAIN FUNCTIONS

Monday, September 4, 9:00 A.M.

Auditorium, Education Building

NORMAN R. F. MAIER, Chairman

9:00 A.M. *The Effects of Cortical Lesions on Emotional and Regressive Behavior in the Rat.* W. C. BIEL and L. I. O'KELLY, Ohio State University.

The relationships between lesions in the cerebral cortex of the rat and emotional and regressive behavior were investigated in this study. Albino rats rated for tameness or docility and systematically handled were divided at 80 days of age into 3 groups by the split-litter technique. The groups were equivalent in terms of sex ratio and tameness ratings. Animals in Groups A (N=28) were subjected to bilateral cortical lesions of varying percentages distributed over different areas of the cortex. Group B animals (N=29) were subjected to identical operative procedure, with the exception of the skull trephination and the cortical insult. Group C (N=31) consisted of normal animals. After a 15-day recovery period (and at a comparable age for the control group) another period of systematic handling was introduced. Following this handling observations were made on behavior in a field situation. Training was then given on a straightaway maze with thirst motivation. The animals were next trained on a one-unit T-maze to a rigid criterion of mastery in one direction, followed by training in the opposite direction to the same criterion. On the trial following the achievement of the second criterion, shock was administered at the choice point. The direction and intensity of activity in the animals after shock were recorded.

Group differences in measures of field behavior were small, although consistent, and relationships between these measures and size of cortical lesions were low. Differences were present between the groups in learning scores on the straightaway and T-maze situation. Learning scores were positively correlated with size of lesions. Reactions of the animals to shock, certain reactions being considered regressive, were found in the case of the operate group to be related to the character of their previous performances. [15 min.]

9:20 A.M. *The Rôle of the Superior Colliculi in the Mediation of Optic Nystagmus.* KARL U. SMITH, University of Rochester.

The present experiment was carried out in collaboration with Mrs. Marjorie D. Bridgman, of Ohio State University.

This study reports results bearing upon the rôle of the superior colliculi in mediating optic nystagmus elicited by movement of the visual field. Records were obtained of the optic head nystagmus of fifteen guinea pigs, by means of a mechanical recording system. Two series of preoperative tests on each animal provided records of the frequency of the nystagmic movements at different effective velocities of visual movement.

Preoperative results were compared to similar data secured on the animals after: (1) unilateral removal of the occipital area of the cortex combined with destruction of the colliculus on the same sides; (2) bilateral removal of the occipital areas combined with destruction of both superior colliculi; and (3) unilateral and bilateral lesions of the superior colliculi.

The postoperative results consistently showed that unilateral destruction of the superior colliculus, whether or not associated with cortical injury, serves to produce defects in optic nystagmus when the visual movement occurs toward the side of the operation. Similarly, bilateral lesions of the colliculus, whether or not associated with cortical injury, produce defects in nystagmus in both directions of rotation.

The results indicate that the subcortical pathways subserving optic nystagmus in a given direction are predominantly segregated in one superior colliculus. Further observations have shown that, with stimulation of only one eye, optic nystagmus can be elicited in only one direction, *i.e.,* with movement toward the side of the non-seeing eye. It would seem therefore that, in the guinea pig, a single retina is represented mainly in the contralateral colliculus. [15 min., slides.]

9:40 A.M. *Category Behavior and Frontal Lobe Lesions in Man.* WARD C. HALSTEAD, Otho S. A. Sprague Memorial Institute and Division of Psychiatry, University of Chicago.

As one aspect of our investigation during the past four years of the behavioral effects of brain lesions in man, we have studied behavior traditionally regarded as involving *abstraction.* For this purpose we have found it necessary to develop test-situations of the grouping or category type. In developing a wide range of such test-situations, we have sought adaptations appropriate for man of Klüver's method of "equivalent and non-equivalent stimuli."

The results of applying one of our test-situations, an object-grouping test, to three series of individuals: (1) normal (control) individuals, (2) individuals with brain lesions outside of the frontal lobes, and (3) individuals with frontal lobe lesions, are reported. In the analysis of our findings both qualitative and quantitative aspects of the test-situation are considered. Tentative conclusions are drawn concerning some aspects of frontal lobe functions. [15 min., slides.]

10:00 A.M. *The Relation of the Electro-Encephalogram to Intelligence Level in the Mongolian Type of Mental Deficiency: Series 2.* GEORGE KREEZER, Cornell University and Dept. of Research, Letchworth Village.

The present study represents part of a series of studies being made to determine whether the electro-encephalogram is of value as an index of psycho-physiological factors associated with intelligence. The use of mentally deficient subjects has important methodological advantages in the investigation of this question. They make available, for example, a wide range of intelligence levels and permit the independent control of mental age and chronological age variables. To provide homogeneous groups, each type of mental deficiency is being dealt with as a separate experimental unit.

In a previous investigation with 50 subjects of the Mongolian type of mental deficiency, a significant correlation was found between occipital alpha index and Binet mental age level, but absence of a significant correlation of mental age with alpha wave frequency. The present paper reports results of an investigation of an entirely new group of 48 subjects of the Mongolian type. A statistically significant correlation was again found of mental age level with alpha index but not with alpha frequency or with delta index. The theoretical significance of these results will be considered. [15 min., slides.]

10:20 A.M. *Bilateral Differences in Brain Potentials from the Two Hemispheres in Relation to Laterality and Stuttering.* DONALD B. LINDSLEY, Emma Pendleton Bradley Home and Brown University.

By means of appropriate amplifying and recording systems brain potentials have been recorded simultaneously from bilaterally homologous areas (occipital) in 48 righthanded, 8 lefthanded and 9 ambidextrous children. Simultaneous bilateral records were also obtained from occipital and motor speech areas of two adult stutterers during periods of rest and non-stuttering and stuttering speech. In all cases the records were studied particularly with respect to the phase relationships of the alpha waves in the two hemispheres and the amount of unilateral, spontaneous blocking that occurred.

The results indicate that the incidence of asynchronism of alpha waves in the two hemispheres is significantly less in the righthanded group of children than in either of the other two groups; the ambidextrous group shows the most asynchronism. The occurrence of unilateral blocking of the alpha rhythm is also greater in the lefthanded and ambidextrous groups; but no significant difference in blocking in right or left hemispheres was found for any one group.

In the stutterers the degree of asynchronism was high but was not increased during stuttering speech. Unilateral blocking of alpha waves was very much greater during stuttering than during periods of silence or non-stuttering speech.

The results will be discussed in relation to integration and control in the two hemispheres, particularly with reference to stuttering. [15 min.]

10:40 A.M. *A Fourier Analysis of the Electroencephalogram from One to Eighteen Years.* JOHN R. KNOTT, University of Iowa, and FREDERIC A. GIBBS, Harvard Medical School.

Left and right occipital, motor, frontal and temporal electroencephalograms have been recorded from a series of 130 normal children and adolescents. By means of the Grass photo-electric wave analyzer these have been transformed into energy *vs.* frequency spectra. The range of frequencies thus analyzed is from 2 to 50 cycles per second. Analysis of the left occipital transforms indicates a pattern at one year exhibiting relatively high energy at frequencies 6 per second, and an absence of any well-defined peaks throughout the entire spectrum. There appears to be a gradual emergence, with age, of energy peaks in the so-called "alpha" (8-12 cycle) and "beta" (18-30 cycle) frequencies, coupled with an increase in the absolute frequency values of these two energy peaks. The occipital transforms will be discussed in relation to transforms of other cortical areas and in relation to adult transforms. [15 min., slides.]

ABNORMAL PSYCHOLOGY

Monday, September 4, 1:30 P.M.
Room 2, Education Building
I. KRECHEVSKY, Chairman

1:30 P.M. *Life Expectancy of Low Grade Mental Defectives.* OSCAR KAPLAN, University of California.

A study was made of the life duration of 768 idiots and imbeciles, exclusive of epileptics, dying in the Sonoma State Home at Eldridge, California, between the years 1917–1939. The average life span of the 424 imbeciles was 26.60 years and that of the 344 idiots was 19.04 years. These figures, however, do not give an accurate picture of the potential life duration of certain types of mental defectives. It is pointed out that 15.80 per cent of the imbeciles and 7.00 per cent of the idiots survived beyond 50 years of age, and that 208 low grade defectives above 50 years of age are still living at the Sonoma State Home. Female imbeciles have a 5.65 years greater life expectancy than male imbeciles, though they live under practically identical conditions. An analysis of the causes of death reveals respiratory disease as the leading cause (especially tuberculosis and pneumonia), and an increase in the number of circulatory failures with increasing age. Relatively few deaths are directly due to failure of the nervous system. Evidence is introduced which indicates that heredity may be a factor in the longevity of mental defectives. It is emphasized that the categories "idiot" and "imbecile" are neither medically nor psychologically homogeneous. On the basis of the results obtained, and assuming that present trends are continued, a substantial increase in the life expectancy of low grade mental defectives is predicted. The social consequences of such an increase are pointed out. [15 min.]

1:50 P.M. *The Etiology of the Deteriorating Psychoses of Adolescence and Early Adult Life.* BARNEY KATZ, University of Southern California.

This study was undertaken to determine the etiological role of cerebral birth trauma in the deteriorating psychoses of adolescence and early adult life. More specifically, it sought to ascertain (1) what traumatizing factors and (2) what factors which determine brain vulnerability are present as etiological factors in the deteriorating psychoses. As cerebral injuries occur during difficult labor and from instrumentation the following traumatizing factors were investigated: (1) mother's age at subject's birth, (2) subject's order of birth, (3) duration of labor, (4) type of presentation, (5) obstetrical complications, (6) pelvic deformity in the mother, and (7) instrumental or version birth. In addition, post-natal factors and heredity were also investigated.

The birth records and family histories of 100 mental patients, all of whom were diagnosed as either " dementia praecox " or " schizophrenia," were analyzed and statistically evaluated. One hundred unselected individuals were similarly investigated and used as controls.

On the basis of the findings the following conclusions seem justifiable: (1) In many instances the deteriorating types of schizophrenic psychoses of adolescence and early adult life appear to originate in a cerebral birth trauma. (2) The traumatizing factors of primiparity, prolonged labor (2nd stage), and instrumental or version birth are of etiological significance in the causation of the deteriorating psychoses. (3) Post-natal head trauma appears to act as a primary factor in some instances, but usually acts as a major precipitating factor in the causation of the deteriorating psychoses. (4) A high incidence of neuropsychiatric conditions was found in the families of the patients as compared to the control group. (5) In 50 per cent of the patients two or more factors are operating and suggest that a multiple of factors are necessary to bring on such a condition. [15 min.]

2:10 P.M. *Mental Efficiency Levels Before and After Shock Therapy.*
MARY PHYLLIS WITTMAN, Elgin State Hospital, Elgin, Illinois.

This is a study of the mental efficiency levels of 170 psychotic patients before either insulin or metrazol therapy compared with their mental efficiency levels after therapy.

A psychometric battery of ten tests, selected or devised as measures of memory comprehension, association, orientation and speed of response, have been given before therapy and an alternate test battery after therapy. In addition, attitude scores before and after therapy have been secured by combining the attitude ratings made by the examiner during the test periods with the ratings made by the charge nurses on the treatment services.

Differences in before and after therapy scores on each of the ten efficiency tests have been studied, and correlated with changes in attitude following therapy. The relationships between the psychiatrists' evaluations of therapy results and the psychological measures of changes in efficiency and attitude following therapy have been determined. The possibility of treatment damage to the central nervous system as indicated by a decrease in mental efficiency following therapy has been investigated and Platner's findings of an amnestic syndrome following combined insulin and metrazol therapy compared with our findings.

The major results of this study include:

A. A definite and statistically reliable improvement in mental efficiency following therapy when contrasted with mental efficiency before therapy.

B. A high correlation between change in mental efficiency following therapy and change in attitude following therapy.

C. A rather low but positive correlation between the psychiatrists' evaluation of therapy results and the differences in before and after therapy scores.

D. A fairly high positive correlation between the psychiatrists' evaluations of therapy results and changes in attitude following therapy.

Suggested explanations for the above findings and plans for further study are given. [15 min., slides.]

2:30 P.M. *A Clinical and Comparative Study of Hysterical Psychopaths.* CATHARINE C. MILES, Yale School of Medicine.

Personality profiles of 25 adolescent and young adult hysterical psychopaths have been analyzed and compared with similar data for 50 other psychopaths and 25 normal subjects in the same age and general ability range. The traits studied include the intellectual (Stanford Binet, Otis speed and power), emotional-social (Bernreuter), and concrete manipulative (Ferguson Form Boards). Comparisons are made in terms of total and partial scores and from factor analysis. The traits and trait relations of the hysterical subjects are examined with respect to their validity as correlates or indicators of hysterical behavior.

The results show a tendency to contrast and inequality within the individual hysterical as compared to the normal profiles. These include evidences of more than average facility and speed in certain simple, familiar performances, relatively poorer capacity for sustained and planful organization, less persistence in carrying out longer and more difficult tasks, an inclination to excessive, overt reactions to environmental demands and a larger number of emotional instability indicators. A "practical" hysterical orientation is suggested. The hysterical subjects show more resemblance to cyclothymic than to schizophrenic subjects.

An attempt is made to relate the findings to theories of the nature and development of hysterical behavior. [15 min.]

2:50 P.M. *Further Experiments of the Neurotic Pattern in Rats.* NORMAN R. F. MAIER, University of Michigan.

The following problems have been explored in order to throw further light on the problem of experimental neurosis in rats: (1) Because of the difference in the effects of different driving stimuli (electric shock *versus* air) on the production of neurotic attacks in rats, attempts were made to determine whether this pattern could be released by appropriate stimuli applied directly. Both normal and neurotic animals were used. (2) A second question which is of vital importance is whether all rats are susceptible to neurotic attacks. This was explored by subjecting stable animals to intensive routines of testing. (3) The problem of inheritance is also of considerable importance particularly to the clinician. Tests on the inheritance of the neurotic pattern have been initiated on a small scale and only preliminary data on this question can be reported. (4) Since brain injury both relieves as well as produces abnormalities, its relation to the neurotic pattern we have reported is of interest. (5) Finally the problem of cures must be raised.

Although data on these points are still in a preliminary state, sufficient evidence is available to indicate the trend of the results. Because these cannot be simply stated they are omitted in this abstract but will be fully presented when the paper is read. Mr. Nathan M. Glaser has assisted in these experiments. [15 min.]

3:10 P.M. *Some Theoretical Considerations Relating to "Experimental Neurosis."* STUART W. COOK, University of Minnesota.

Many investigators are simultaneously studying the phenomenon of "experimental neurosis." If at this early date in the history of the problem, agreement is reached upon certain matters of a non-experimental nature, much confusion in interpretation may be avoided. Likewise, if hypotheses arising from the common findings of previous experiments are formulated at this time, future investigations may be brought to bear more directly upon early misconceptions. This paper discusses certain problems having to do with definition, criteria for diagnosis, classification, level of explanation and comparative interpretation, and presents hypotheses suggested by the pioneer work on the problem.

In order to avoid the confusion likely to arise as a result of previous connotations of the word neurosis, "experimental neurosis" is defined so as to include *all* persistent abnormal behavior, experimentally produced. Quotation marks around the term are intended to call attention to this definition. The problem of identification of "experimental neurosis" suggests the necessity for criteria of selection; those advanced here center around a *persistent change* toward maladaptive response. The fact that different syndromes of abnormal symptoms have been observed raises the question of classification. Arguments are given for grouping in behavioral terms rather than on the basis of vaguely substantiated neurological concepts. With regard to comparative interpretation the position is taken that analogies drawn between lower and higher species on the basis of specific symptoms are likely to be misleading. Comparative analyses in terms of the normal behavior of the species and the total picture of the abnormal reaction are supported. Finally, hypotheses concerned with characteristics of the precipitating situation and the reactive predisposition of the animal are discussed together with the experimental sources from which they are drawn. [15 min.]

3:30 P.M. *Experiments Relating Freudian Displacement to Generalization of Conditioning.* NEAL E. MILLER, Institute of Human Relations, Yale University.

In the first experiment, pairs of albino rats were trained to commence striking one another, at the signal of a mild electric shock, in a manner similar to the way in which rats strike at one another when normally fighting. This behavior was reinforced by turning off the shock as soon as the rats struck one another vigorously. After this training, a small celluloid doll was placed in the arena along with each *pair* of rats. Under these conditions the animals struck at each other and not at the doll. When placed *one at a time* in the same apparatus, however, they struck at the doll. A rat first attempted, in short, to strike the other animal, but when this was prevented by the absence of that animal it struck the doll. Viewed in Freudian terms this should indicate displacement; in stimulus-response terms it would be generalization of conditioning or transfer of training. (Miss Maritta Davis helped perform this experiment.)

In a second experiment, performed with the assistance of Miss Inez Aaronson, it was found that, if thirsty rats were trained to run down an alley to water and then satiated till they would drink no more, they would run down the alley faster to *water* if *hungry* than if not hungry. Similarly, with the assistance of Mr. Jacob Boldstein, it was demonstrated that animals trained to run for food and then satiated would run more rapidly to food on the correct arm of a T maze when frightened than when not frightened. Again the results can be interpreted as being analogous either to generalization of conditioning or to displacement of motivation.

Psychoanalytic observations on displacement suggest that consciousness or verbal control should have certain effects upon the generalization of conditioning. [15 min., slides.]

3:50 P.M. *Anxiety and Learning.* O. H. MOWRER, Yale University.

The efficacy of anxiety as a motivating and reinforcing agent is tacitly acknowledged in many practices commonly employed in both animal training and human education. The principles involved in these practices have not, however, been subjected either to refined theoretical analysis or to carefully controlled laboratory investigation. In laboratory learning experiments with adult human subjects, the factor of motivation (and reinforcement, conceived as the response-fixating influence of motivation reduction) is usually defined vaguely, if at all; whereas, in animal experiments motivation is usually too narrowly defined, being limited to hunger, thirst, sex, electric shock, or the like. Anxiety, defined as anticipation of painfully intense stimulation (of either external or internal origin), appears to exercise an important influence in actually shaping human and infra-human behavior alike. Explicit recognition of the psychological importance of anxiety offers the hope of bridging the gap between so-called normal and abnormal behavior, of accounting for many now perplexing social phenomena, and of eliminating some of the contradictions and imperfections of current learning theory.

In the present report, an attempt will be made (1) to state certain principles concerning the conditions of anxiety formation and anxiety dissipation, (2) to formulate a few major predictions that derive from these and correlative psychological principles, and (3) to describe some experimental investigations that have been aimed at testing the validity of these predictions.

Reference will also be made in passing to some of the difficulties, both systematic and practical, that arise (1) in connection with the detection and independent measurement of anxiety and (2) in connection with the use of animal subjects in experimentation of this kind. [15 min., slides.]

ANIMAL LEARNING

Monday, September 4, 1:30 P.M.
Room 521, Hydraulic Laboratory
EDWARD C. TOLMAN, Chairman

1:30 P.M. *A Further Study of One Trial Learning in Rats.* **BRADFORD B. HUDSON**, University of California.

In a preliminary experiment, rats were shocked once on the mouth by a condenser discharge as they ate from a metalic food holder, attached to the center of a black and white striped cardboard pattern. The bait used was their regular diet, moistened instead of dry. Monthly tests, without reinforcement, revealed good retention of fear in several rats for six months.

In the present experiments two variables were investigated for their effects on the learning and retention of this single experience: (1) The effect of age at the time shocked; (2) The effect of the size of the temporal intervals interposed between the shock experience and the start of the series of daily test trials.

Rats, 30, 50, and 70 days of age, were shocked in their living cage and then tested daily for five minutes, beginning two weeks later. Their regular diet was always available except during the shock and test trials. In these groups, 47, 43, and 14 per cent, respectively, failed to show the characteristic "caution" in their approaches to the bait or the equally significant withdrawal responses. Of those rats which both learned and showed retention over this period, the average number of trials required to produce extinction was 3.0, 3.2, and 3.5, respectively.

These trends become significant when considered in conjunction with the results of a comparable group No. 1 in the study of the second variable, the effect of time intervals. Two groups, age 240 days, but in this case motivated, were shocked and then tested daily, beginning at 2 and 6 weeks respectively. In group 1, 100 per cent learned and resisted extinction for an average of 10.6 days. In group 2, 78 per cent learned and resisted extinction for an average of 3.4 days. [15 min., slides.]

1:50 P.M. *Learning Ability of Rats Cured of Rat Pellagra.* **D. E. SMITH** and **L. B. PETT**, University of Alberta.

In some studies of the effect of a diet deficient in the vitamin B_2 (G) complex, the learning ability of deficient animals was significantly inferior to that of normals; in others this was not found. These differences in results are associated with differences in duration of the deficient diet, and may have been due to variations in the extent of recovery. The present study proposed to investigate the learning ability of rats in which suitable recovery from the effects of the vitamin deficiency was attained before learning began.

Seventeen rats in the control group were fed from weaning a complete, purified diet. Twenty-six rats in the experimental group were fed from weaning a similar purified diet, deficient only in vitamin B_6, until the symptoms of rat pellagra (growth retardation, scaly dermatosis, etc.) fully appeared. Massive doses of the vitamin were then added to the diet. When the animals of the experimental group attained two-thirds the average weight of the control group, and other symptoms of deficiency had disappeared, both groups began training on a 4-unit water maze. The groups were equated as to sex, and the split-litter technique was employed.

The two groups were equal in learning ability, as measured by the total number of trials required, and the total number of errors made.

These results suggest that any loss of learning ability which may occur in deficiency of the vitamin B_2 (G) complex or its components is restored when the animal recovers from other effects of the deficiency. Therefore, placing the animals on a normal diet before learning is complete, complicates in an uncontrollable manner studies of such loss of learning ability. [15 min., slides.]

2:10 P.M. *Transfer of Training in the Mastery of an Antagonistic Habit After Varying Intervals of Time.* M. E. Bunch, Washington University.

The question investigated is whether or not the negative transfer effect shown in the learning of a second of a pair of tasks is a function of the amount of time elapsing before the second is learned. Simple right and left position habits in a one unit "T" water maze were chosen as the problems to be learned because of the readiness with which white rats establish such habits and the very great likelihood of the occurrence of negative transfer in learning the second task immediately following the first. Each task was learned by the method of massed practice. The amount of the detrimental influence, or negative transfer, that occurs when the two are learned in immediate succession is determined and compared with the amount and character of the transfer effect that occurs under different temporal conditions. The intervals of time after which the transfer effect from the first to the second was measured are: immediate succession, 2, 7, 14, and 28 days.

The results indicate that the negative transfer effect of the first habit on the second when the two are learned in immediate succession decreases rapidly with the passage of time and becomes positive in character after intervals of 14 and 28 days. Thus, whether the first of a pair of antagonistic habits actually interferes with the second is a function of the interval of time between them.

The relation of the results to the studies of the transfer-time question involving a positive transfer effect from the beginning will be discussed. [15 min.]

2:30 P.M. *Rate of Extinction in Maze-bright and Maze-dull Rats.* W. T. HERON and B. F. SKINNER, University of Minnesota.

The following experiment tests the assumption that differences in maze performances are due in part to differences in the rate of extinction of "wrong" responses. Fifty maze-bright and forty-five maze-dull rats, both male and female, of the F_{14} generation segregated on the Minnesota automatic maze, were tested for differences in rate of responding under periodic reinforcement and in the rate of subsequent extinction. The response studied was pressing a lever; the reinforcement was with food. A new apparatus permitting the simultaneous study of twenty-four rats was used. Approximately equal numbers of bright and dull rats were compared in four experimental sub-groups. Records were analyzed graphically and statistically. The rate of responding under periodic reinforcement, previously shown to vary with hunger, exhibits a significant difference between males and females in favor of females and between some of the sub-groups of bright and dull rats. The mean extinction curve for the whole group is typical of the process of extinction as previously reported. The probability that the bright and dull rats were drawn from different populations with respect to rate of responding during extinction passes the .05 level in some sub-groups in favor of a greater rate for the bright rats. But this is also the direction of the difference in rate under periodic reinforcement, and both differences are probably due simply to the different degrees of drive obtained in the two strains with a common method of feeding. The negative acceleration during extinction is not significantly different. The curves can be superimposed with the aid of a slope factor. The difference in maze performance cannot, therefore, be due to the rate of extinction. Although other aspects of learning remain to be tested, the experiment suggests that motivation may be a more important factor. [15 min., slides.]

2:50 P.M. *Analysis of Learning Data Using Psychologically Meaningful Parameters.* HAROLD GULLIKSEN, The University of Chicago.

When individual learning curves are fitted to discrimination learning data (*e.g.*, light-dark discrimination), three psychologically meaningful parameters of the individual learning curves may be calculated. These parameters lend themselves more readily to psychological interpretation than do the usual scores of total time, total errors, or total trials. These three parameters are measures of: (1) initial preference (*e.g.*, preference for the light or for the dark); (2) a criterion of degree of learning; and (3) the individual's learning ability.

These individual learning curve parameters were used in analyzing some data obtained by Krechevsky. Comparisons are made between groups of normal and of operated animals trained on a light-dark discrimination. This analysis shows the effect of punishment and of cortical operation on learning ability and on initial preference. [10 min.]

3:05 P.M. *A Further Study of the Cat in the Puzzle-Box.* GEORGE PLANT HORTON and EDWIN R. GUTHRIE, University of Washington.

The authors have previously reported that the escape-movements of animals from a puzzle-box demonstrated habits which were stereotyped to an extent not formerly emphasized in discussions of this problem. To answer the question of what conditions lead to the stereotyping of the escape-responses three different release mechanisms were employed. In the first condition release was effected by tilting a vertical pole in any direction; in the second condition release was effected by cutting the light beam of a photo-electric cell as the beam passed through the *only* hole in the floor of the box; in the third condition release was effected by cutting the light beam of a photo-electric cell as it passed through *one* of the *many* equally spaced holes in the floor of the box. Since, for photographic purposes the box was illuminated with three 1,000 watt photoflood bulbs, the beam of the photoelectric cell was not discernable. The records consisted of direct observations by the experimenters, of *one* continuous and several discontinuous motion pictures, and of still photographs of the escape movement obtained by a camera operated electrically by the release mechanism.

The first two conditions offered the animal more orienting stimuli than did the third. The responses obtained under these two conditions showed greater stereotyping in the still pictures than did those obtained under the third condition. However, the experimenters' observations and the cinematographic records showed many highly stereotyped serial responses in all cases. The still pictures being only a cross-section of the total action had failed to demonstrate these responses in the third condition. Reasons for these differences in the still pictures will be discussed. [15 min., slides.]

3:25 P.M. *An Apparatus for the Study of Alternation Behavior in the Cat.* WILLIAM E. KAPPAUF, University of Rochester.

A lever-type apparatus has been developed for the study of alternation behavior in the cat. Two pedals are arranged in the response box and the animal is required to press them in the order RLRL, or RRLL, etc. A correct response to either pedal involves pushing it to the floor through a distance of about 20 mm. The animal receives a shock if it presses one of the pedals out of sequence. Food is delivered at a position midway between the two pedals after each correct response or, if desired, only upon the completion of a series of required responses. The apparatus is automatic in operation, the alternation, shock and feeding being controlled through a set of rotating selector switches.

Preliminary results on the training of a group of six animals will be reported. [10 min., slides.]

3:40 P.M. *Symbolic Reactions for Delayed Reward and Delayed Response in Chimpanzees.* AUSTIN H. RIESEN, Yale Laboratories of Primate Biology.

A critical review of the literature on animal learning fails to reveal any instance of successful discrimination learning with delayed reward when the stimuli involved lacked differentiating spatial components. Non-positional delayed response has never been demonstrated in animal subjects except after special and prolonged training. Hypotheses are formulated to account for facts of learning with delayed reward and performance of delayed response. Certain predictions as to the outcome of visual discrimination learning with delayed reward by trained and untrained chimpanzee subjects are made.

A discrimination apparatus is described which provides for the equalization of frustration of delay while making possible variation of the interval between the disappearance of the visual stimuli and the occurrence of reward or frustration. Control subjects were found incapable of learning successive red *vs.* green and green *vs.* red discrimination habits when the differential response to the color stimuli was followed by the reward or frustration consequences of the response at an interval greater than 2 or 4 seconds. Experimental subjects received special training designed to establish symbolic reactions (specific anticipatory eating movements) to the color stimuli. Following such training, rate of learning by these animals was independent of the interval of separation between differential response and reward or frustration. Intervals up to 20 seconds were used in tests. Learning was extremely rapid, many habit reversals being learned with no errors occurring after an initial trial, which was sometimes a rewarded and sometimes a frustrated choice.

The results, which parallel instances of one-trial learning mediated by speech symbols in human subjects, are in agreement with the hypotheses concerning the symbolic nature of reaction habits essential to delayed reward learning and delayed response performance. [15 min., slides.]

TESTS AND MEASUREMENTS

Monday, September 4, 1:30 P.M.
Auditorium, Education Building
FRANK N. FREEMAN, Chairman

1:30 P.M. *A Study of Personal Variation in Hand-Arm Steadiness.*

CONSTANCE LOVELL, The University of Southern California.

As a preliminary study of the value of measures of personal variation in describing individual performance, hand-arm steadiness tests were made for fifty university students to determine (1) if degree of personal variation was specific to the time interval used in calculating results and (2) if the variation found in each individual's series of trials would be relatively consistent over a period of time. The Seashore Photo-Electric Target Register, a device for the measurement of steadiness, was wired for kymograph recording to allow for the division of each trial into fractional parts. Results computed in terms of three different time periods indicated that variation was not highly specific to time interval. Correlations between the variation in two series of trials, a month apart, ranged from +.49±.08 to +.74±.05 (depending on the measure selected). [10 min.]

1:45 P.M. *A Test Standardized on Pueblo Indian Children.* PHILIP H. DUBOIS, University of New Mexico.

Most of the comparisons of the abilities of Indians and whites have been made with tests utilizing material common in white culture and standardized on white groups. In this study the procedure has been reversed.

Practically all of the children in 26 of the 27 day schools of the United Pueblos Agency were asked to make two drawings, one of a horse and the other of a man. Sex, age and grade placement of each subject were recorded and each was rated by the teacher for general intelligence.

For 955 subjects information regarding age was exact. Their horse drawings were scored on an *a priori* scale of 73 points, devised by analogy with the points of the Goodenough Intelligence Test. Of the 73 points, 60 showed fairly regular increases in the percentage of children succeeding with the point at successive ages and were retained in the revised system of scoring.

Reliability as determined by the correlation of two sets of 30 items ranged from .64 for 4-year-olds to .90 for 9-year-olds, when the age groups were considered separately. Reliability was also established by the test-retest method.

Validity was established by correlation with grade placement and other criteria. For Pueblo Indian children the horse drawing test seems to have greater validity as a measure of mental ability than the Goodenough.

Sex differences are considered. Attainment of groups of white children on the test is also given. The assistance of Miss Marian Eller is acknowledged. [15 min., slides.]

2:05 P.M. *Results from the Use of Special Tests for Automobile Drivers.* CLARENCE W. BROWN, University of California.

In recent years conflicting reports have been made concerning the value of psychological tests for automobile drivers. Misunderstandings have arisen because a few workers in this field have made claims for the tests which are primarily traceable to the workers' personal zeal rather than to an adequate body of experimental fact. Furthermore, progress in evaluating the tests has been slow because a record, over a period of several years, of certain driving habits of a large number of tested and untested drivers has not been available for analysis.

For the past three years the state of California has administered a battery of psychological tests to a large number of drivers. Eighteen different measures on special sensory and motor tests, together with facts of personal history were obtained from each driver. Two general procedures were used for evaluating the tests. In the first, the capacity of the tests to measure driving ability was ascertained by comparing the performances on the tests of groups of good and poor drivers. In the second procedure the actual road performances of tested and non-tested drivers were compared both before and after the administration of the test.

Two general conclusions seem warranted by the facts: (1) certain of the measures reliably differentiated between groups of good and poor automobile drivers, and (2) the administration of the tests caused a marked improvement in the driving performance of those tested. [15 min.]

2:25 P.M. *A Factor Analysis of Mechanical Ability Tests.* WILLARD HARRELL, University of Illinois.

Mechanical aptitude tests were given to ninety-one cotton mill machine fixers. Results were treated by the Thurstone centroid method of factor analysis.

The Minnesota tests of mechanical ability, the MacQuarrie test of mechanical ability, Wiggly Block, and Stenquist I picture matching test were included along with thirty other variables. Other manual and spatial tests, verbal tests, and personal data composed thirty variables chosen to aid the identification of factors guessed to be present in the above tests.

Five factors were isolated, these were: perception of detail (*P*), verbal relations (*V*), visualizing spatial relations (*S*), manual agility (*A*), and youth (*Y*). Identification of *P* is the least certain of the five. The first three of these factors, *P, V,* and *S,* have previously been identified by Thurstone and others. Oblique axes were used since the abilities, at least for the group studied, are correlated.

The Minnesota tests comprise not merely one group factor but two, *P* and *S,* while *A* is present to a high degree in one of the putative mechanical ability tests—the MacQuarrie dotting sub-test. Inclusion of other variable might show other factors in "mechanical ability" tests but the representative nature of the present battery indicates that *P* and *S* are the most important.

Certain group paper-and-pencil tests measure each of the factors present in the manual tests.

The results contain a suggestion that insight into mechanical relations is bound up in factor *S,* while factor *P* is more routine. This indication comes from the dropping out of *S* which was present in the assembly test, when the test was repeated, leaving only factor *P.* This conclusion, however, is not definite, as the test was scored the first time for accuracy, and the second time for speed. [15 min.]

2:45 P.M. *A Comparative Study of Retest Ratings on the Original and the Revised Stanford Binet Intelligence Scales.* GRACE MUNSON and MILTON A. SAFFIR, Bureau of Child Study, Chicago Public Schools.

The publication of the Revised Stanford Binet has raised the question of the comparability of the ratings on the revised and the original scales. The revision has also added new opportunities for studying the constancy of the I.Q. The authors have studied these questions through analysis of the results of the use of each of these scales for retesting two groups of children—approximately 1,000 children in each—using the old Stanford Binet for the first test. All of the retests as well as the first tests were administered by trained psychologists. The children tested were referred because of school problems. The statistical constants indicate that the two groups were very similar, and for the most part the I.Q.'s were in the 50–100 range.

Comparison of the two scales used for the retest has been made both through test-retest correlations, and by the study of the amount and direction of change in I.Q. The data were classified to study the change in I.Q. with each of the tests as related to original C.A., time interval between test and retest, and original I.Q. level.

Analysis of the results indicates a drop in I.Q. for retest with either scale, but a smaller drop when the Revised Stanford Binet was the scale used. The drops found were less than those previously reported or predicted. Change in I.Q., ignoring *direction* of change, is no greater with the revised scale than with the original one. Significant data were found concerning the relationship of change in I.Q. to such factors as C.A. at time of first test, I.Q. level, and time interval between tests, for retest with the new as well as the old Stanford Binet. [15 min., slides.]

3:05 P.M. *Further Data on the Measurement of General English Vocabularies.* ROBERT H. SEASHORE, Northwestern University.

Extensive studies with a shorter revision (Form 1) of the Seashore-Eckerson General English Vocabulary Test indicate that:

(1) *Unabridged* dictionaries are necessary to provide an adequate sampling for the measurement of the total size of individual vocabularies.

(2) Such a dictionary (Funk and Wagnalls) contains approximately 167,000 *basic* (marginal, heavy type) entries, plus 204,000 *derivative* (other parts of speech, compound words) entries, or a total of 371,000 words. The remaining 87,000 multiple meanings and 13,000 variants in spelling are not ordinarily considered separate words.

(3) The average college undergraduate can *recognize* the commonest single meaning of approximately 61,000 basic and 96,000 derivative words, or a total of 157,000 words. He can *use* about 153,000 of these in illustrative sentences.

(4) The *use* vocabularies commonly cited for famous authors (Shakespeare, 15,000 words) have not counted derivatives as separate words. Neither do they take into account additional words which the writers *could have used* on topics other than those included in their formal writings. The English language was also much smaller at the time when Shakespeare wrote.

(5) Knowledge of the single commonest meaning of a word is related to knowledge of additional multiple meanings of the same words. ($r=.68$.)

(6) Additional data include measurements of reliabilities, college norms for 20 minute and unlimited time conditions, intercorrelations with tests of reading comprehension, reading speed, language usage, general intelligence, and college grades, and preliminary results with grade school children.

Lois D. Eckerson, Raymond Starmann, and George D. Lovell have collaborated in these studies. [15 min.]

3:25 P.M. *The Measurement of Visual Depth Perception.* **Neil Warren**, University of Southern California.

Correlation of the results obtained by measuring the same subjects with two standard tests of binocular depth perception indicates that the tests do not give equivalent measures. While the ocular features of the two procedures are fundamentally similar, certain extrinsic factors influence the results. The lack of correspondence between the tests, both of which are widely used, raises questions about the practical values of such measurements.

Consideration is given to the nature of individual differences in visual depth perception and to the principles upon which tests for such differences should be designed. [10 min.]

3:40 P.M. *The Relationship of Single Binocular Perception to the Stimulation of Retinal Corresponding Points.* **Eleroy L. Stromberg**, Oklahoma A. & M. College.

Previous evidence presented by the author indicates that the two eyes do not necessarily fixate upon the same point in the printed line during reading. Frequently, the variation is as much as ten or twelve letter spaces. In the same line of print, the fixation points often reverse their relative positions. That is, whereas the left eye leads at the beginning of the line, the right eye may be the leading eye at the end of the line.

This variation in fixation points suggests either that: (1) The stimulation of corresponding points of the retinae is not necessary for single binocular perception, or that, (2) Vision is suppressed in one eye during reading perception.

These data support the former conclusion. [10 min., slides.]

3:55 P.M. *Interrelationships among Criteria of Reading Ability and Measures of Visual Functions.* **Hubert C. Armstrong**, Institute of Child Welfare, University of California.

The problem is to determine which visual functions are associated with reading disability. Standardized tests were used to measure reading ability and mental maturity. Visual functions were measured by (1) standard optometric instruments, (2) eye movement photography, (3) the haploscope, and (4) a calibrated stereoscope with standard cards. The measures obtained with the number of methods employed shown in parentheses included: visual acuity (2); refractive error (1); vergence (3); fixations (1); speed (1); binocular fusion (1); accommodative convergence relationships (3); stereoscopic vision (1). The results, which are at this writing incomplete, include (a) statements of remeasurement reliability. Correlation coefficients range from .00 to .80; (b) interrelationships of ocular variables with the criteria are generally too low to be significant. Selected positive relationships will be presented. [10 min.]

4:10 P.M. *A Test for Types of Formal Creativity.* **Ernst Harms**, U. S. Department of Agriculture, Washington, D. C.

Can we test the formal creativity expressed in any artistic activity and distinguish clear types? Three thousand and five hundred children (three age groups 6–9, 10–14, 15–18) of New York City, 200 students of George Washington University, Washington, D. C., and 600 adults were tested by giving them series of 10–15 suitable words (mostly various actions) to express their contents in a single line. This test was repeated with the same individuals and variated for getting clear definite information.

There could be for all age stages distinguished five general types (1) symbolic: walking-footprints, love-heart; (2) spacial: positive-up, negative-down; (3) propensitive: final directed; (4) typographical: short typelike lines; (5) monographic: slight variations of a similar line. Further types: (a) fantasiful: for everything on other formal expression; (b) fantasiles: monotone similar lines; (c) neurotic: about which a special publication will be prepared.

Average (approx.) for all ages: 25% symbolic, 15% spacial, 15% propensitive, 10% typographic, 10% monographic. Rest unclear. Symbolic higher among children under 10 and among adults over 25. Tired children lean to symbolic. Spacial and propensitive highest at 15–25. [15 min., slides.]

RESEARCH AND INSTRUCTIONAL FILMS

Monday, September 4, 8:00 P.M.
Auditorium, Education Building
HAROLD E. JONES, Chairman

Note: The showing of films will not be accompanied by verbal descriptions, since proper titles in the films will be adequate.

Song-Isolated Roller Canaries. MILTON METFESSEL, University of Southern California. [Sound, 15 min.]

Experimentally Produced Neurotic Behavior in the Rat. NORMAN R. F. MAIER, University of Michigan. [25 min.]

Symptoms in Schizophrenia. JAMES D. PAGE, University of Rochester. [25 min.]

An Experimentally Produced " Social Problem " in Rats. O. H. MOWRER, Yale University. [8 min.]

One Trial Learning in Rats. BRADFORD B. HUDSON, University of California. [10 min.]

Testing Animal Intelligence. CARL J. WARDEN and G. M. GILBERT, Columbia University. [18 min.]

The Neurophysiological Integration of the Starfish. WILBER S. HULIN and ARTHUR R. MOORE, University of Oregon. [10 min.]

Artificial Oestrus in the Guinea Pig Without Ovaries and Uterus. JOHN P. SEWARD, Connecticut College. [5 min.]

A Motion Picture Study of Behavior of Cats After Frontal Lobe Lesions. KARL U. SMITH, University of Rochester. [10 min.]

Instruction in Animal Psychology. WILLIS D. ELLIS, University of Arizona. [10 min.]

Experimental Studies of the Learning Process in Children. HAROLD E. JONES, University of California. [15 min.]

PSYCHOMETRICS

Program Arranged by the Program Committee of the
Psychometric Society
Tuesday, September 5, 9:00 A.M.
Room 2, Education Building
LEWIS M. TERMAN, Chairman

9:00 A.M. *Selecting Salesmen by Personal History Items: Methods and Results.* ALBERT K. KURTZ, Life Insurance Sales Research Bureau, Hartford, Connecticut.

Twenty-three personal history items (occupation, number of dependents, living expenses, etc.) were studied to determine their value, singly and combined, for predicting success in selling life insurance.

Two criteria (whether or not the salesman remained in business at least one, or two, years) related to turnover; and two (total life insurance sales in first one, or two, years) related to selling ability of survivors.

A scoring system, based on all four criteria, was developed for each item. Six items, which individually showed high validities, formed a preliminary rating chart. Seventeen remaining items were added in turn, and the 68 possible correlation coefficients (based on approximately 500 to 3,000 cases) were computed between every such 7-item rating chart and each criterion. Four more items which raised validity coefficients by relatively large amounts were included, forming the 10-item *Rating Chart.*

Each of 10,111 salesmen was scored on this 10-item *Rating Chart.* Boundary lines for *Rating Chart* scores were set up for each age so men classified in, for example, the *B* group, would average a certain predetermined amount of first year sales—regardless of their age.

The effectiveness of the *Rating Chart* is shown by figures on both the original 10,111 salesmen and two alien populations of 743 and 878. The proportion of men with *A* ratings on the *Rating Chart* who continue working at least one year is nearly double that of *E* men. Of survivors, average sales of *A* men are triple those of *E* men. Consequently, total first year sales are five times as great if *A*, rather than *E*, men are hired.

Time for answering *Rating Chart* questions: 10 minutes; scoring time: 5 minutes.

Dr. Rensis Likert made many valuable suggestions regarding analytical procedures. [15 min., slides.]

9:20 A.M. *Improved Cluster-Orthometrics.* ROBERT C. TRYON, University of California.

An improved technique for estimating communalities is made available, one involving the use of the expression: $\Sigma r_{ak}/\Sigma r_{bk}=h_a/h_b$, for the congruent variables, *a* and *b*. The orthometric analysis of the augmented correlations is abandoned in favor of a new technique in which the coördinates of each test are determined from the unaugmented correlations, the operations being focused around certain *key centroid tests.* Orthogonal axes, which may exceed three, are successively introduced until mean residual correlations between the key centroid tests vanish within their standard error, the formula for which is presented. Tests of goodness of fit are thus carried along routinely with the analysis. Calculational work is minimal, requiring for a twenty variable problem, for example, only two sheets of mechanic's bond. For a pictorial and quantitative description of the relationships between test clusters, geometric figures of three or less dimensions are employed, the tests being depicted in terms of their augmented coördinates. Where the number of axes exceeds three, different "alternative" orthometric figures, derived from simple rotational procedures, are employed. In each figure the augmented total variance of the tests there depicted is unity, and no test of fit is required in contrast to the earlier method. Thus the entire organization of test clusters, whatever the number of orthogonal axes required in the analysis, is made visually apparent and is quantitatively described in alternative spaces of three dimensions. [15 min.]

9:40 A.M. *Measuring Interests.* JOHN C. FLANAGAN, Coöperative Test Service, New York City.

One of the earliest systematic attempts to measure interests was that of Truman Lee Kelley, reported in his monograph, *Educational Guidance,* twenty-five years ago. Since that time, many other investigators have contributed to this field. The progress made by these investigators is probably best illustrated by the Strong Vocational Interest Blanks. The scores on these scales are determined by the individual's *expressed* preferences. Such responses are likely to reflect wishful thinking, or "expected" answers.

The present report describes another approach to the measurement of interests. A test composed of items in six major fields measuring the extent to which information concerning events of the preceding year has been acquired and retained was administered to several groups, principally college sophomores. The profile of relative scores provides measures of the concrete results of actual interests operating over a period of time and, therefore, reflects functioning interests in terms of objectively measured results. The pattern which becomes apparent when the student's six sub-scores (expressed in comparable units) are compared will give an indication of the pattern of his relative interests in these fields as evidenced by measures of the results of his actual behavior during the past year.

To study the validity of these measures, the results on the test were compared with other data, such as statements of: Field of major interest, Types of activities in which the individual is most interested, Reading preferences with respect to selected periodicals, Professional goal, Major field of study, and scores on achievement tests and the Strong Vocational Interest Blank. A summary of the findings is reported. [15 min.]

10:00 A.M. *Sampling in Factor Analysis.* QUINN MCNEMAR, Stanford University.

In order for the factorial methods to be more useful it is necessary that we know something about sampling errors as they affect the dependability of axes, the stability of test projections on the axes, and the number of factors to be extracted. Different factorial patterns derived from different groups, or produced experimentally (*e.g.,* by practice), or found by following the same group from one age to another, cannot at present be adequately evaluated in terms of sampling variations. It is the purpose of this paper to present empirical data on the stability of test projections. 'Scores' for 2,500 'individuals' on seven 'tests' were set up in terms of known common elements in such a way as to permit the following combinations: six tests with three factors, five tests with two factors, and five tests with one factor. For 25 subexamples of 100 each, three sets of analyses, according to the above combinations, were carried out. This report is concerned primarily with the results from these artificial data. Another study involving nine variables is in progress in which 35 subsamples of 200 each have been drawn from a supply of 7,000 College Entrance Examination Board blanks. The general findings for the completed part of this second study will be indicated. [15 min., slides.]

10:20 A.M. *Two Statistical Notes.* TRUMAN L. KELLEY, Harvard Graduate School of Education.

The idea that the variability of a sample of means may be estimated from a single sample of subjects giving a single mean is, historically, of rather recent origin. There is need for the teaching of this concept, as a type concept, early in a course in measurement. The writer has always found difficulty in presenting the subject to beginning students. Herein is a somewhat novel derivation of the formula $\sigma_M=\sigma/\sqrt{N-1}$ which may have some merit in connection with elementary instruction.

The standard error of a multiple regression coefficient is not readily given by the constants derived in the usual abridged methods for determining regression equation constants. Herein are presented certain procedures and approximation formulas for quickly obtaining serviceably close estimates of the standard errors of multiple regression coefficients. [15 min.]

10:40 A.M. *The Limits of Random and Systematic Practice Effects.* EDWARD B. GREENE, University of Michigan.

A correction for attenuation, using a correlation between supposedly equivalent forms, is often recommended on the assumption that low correlations indicate random variations in scores. This assumption may be in error to a marked degree, for systematic variations are often present.

Results of one experiment were analyzed to determine the limits of systematic variations. Four trials of six tests of motor abilities and six of comparison were given to 394 high school boys. With practice all of the tests showed marked and related changes in mean scores, standard deviations, coefficients of variation, correlations between adjacent trials and factor loading patterns. These results, together with subjective reports, indicate large systematic variations in performance. The conclusion is reached that corrections for attenuation are unjustifiable when based on data similar to those discussed. The probable limits of random and systematic variations are outlined for three commonly met situations. [10 min.]

SOCIAL AND POLITICAL PSYCHOLOGY

Program Arranged by the Program Committee of the Society for the Psychological Study of Social Issues

Tuesday, September 5, 9:00 A.M.

Auditorium, Education Building

Kurt Lewin, Chairman

Fifteen minutes allowed for discussion of each paper.

9:00 A.M. *A Socio-Psychological Case Study of an Institutional Crisis: A Methodological Exploration.* Goodwin Watson, Teachers College, Columbia University.

How does the psychologist, as distinct from reporter, historian or sociologist, analyze complex social processes? Few models can be found in the literature. A case study is here attempted of the three months of excited activity which followed the announcement that New College would be discontinued. In some respects that pattern of events paralleled, on a small scale, the course of attempts at political revolution. Time sequences, records of group meetings, interviews with all leaders concerned, and extensive personal data concerning many participants, have been studied to discover social psychological patterns, relationships and hypotheses. The central purpose of the paper is methodological, and is concerned to find techniques of analysis that may prove fruitful in further study of the psychology of social change. [15 min.]

9:30 A.M. *A Critique of the Telic Continuum and the J-Curve Hypothesis.* Arthur Jenness, University of Nebraska.

The concept of the telic continuum has made possible greater precision in the definition of norms of behavior and of the degrees of conformity to these norms which prevail in given populations. Ambiguity still arises from two confusions: (1) the telic continuum is sometimes considered as one of degrees of fulfillment of the purpose of the act and sometimes as one of degrees of conformity to a rule, (2) some writers consider the term "norm" to be synonymous with "frame of reference." A conceptual system will be offered which embraces all of these concepts, but overcomes the difficulties mentioned.

The J-shaped distribution is not peculiar to institutional behavior; the data of clinicians and physicians demonstrate that the shape of the distribution may be due to the manner in which the criteria of behavior are selected. Allport contends that the double J-shaped curve is positively accelerated on both sides and is not an ordinary skewed curve, since the latter is negatively accelerated as it approaches the mode. In some of Allport's double J-curves, the lack of negative acceleration is an artifact of his statistical technique.

Where the prescribed behavior is represented on an "empirical" continuum (*e.g.*, arriving at work on time), the mode of the distribution frequently falls at a point above or below that stipulated in the rule. In such cases the curve may be considered as a curve of errors in approximating the prescribed behavior. Thus the modal time of arrival at work is usually before the stipulated time; this may be considered as a constant error, the magnitude of which is a function of the severity of the penalty for late arrival. [15 min., slides.]

10:00 A.M. *Selection for and Contagion of Paranoia in Organized Groups.* HARRY C. STEINMETZ, San Diego State College.

The purpose of this paper is to delineate paranoid condition in such terms as shall aid in distinguishing between supporters of a movement whose motives, however derived, are under the direction of principle and analysis, and those whose motives, ushering them into a congenial movement or derived from its development, are unamenable to such control. Normalcy, in its medical connotation for psychology, is relevant to functional group membership. Misunderstood and loosening bourgeois relationships cultivate paranoid conditions in relief from conflict and uncertainty (*Mein Kampf,* "the curse of objectivity") and, conversely, bourgeois intellectuals can readily classify as paranoid those whose insight reveals the conspiratorial nature of social controls, who rise above decadent cultural limitations. Conspiratorial activities foster the symptoms, and these induce the condition. The sources are psychiatric and fascist literature, together with experiences in political and labor groups, and study of institutional cases. A significant cue is to be found in a compulsive self-exemption from group obligations from a projected delusion of unique worth. [15 min.]

10:30 A.M. *The Analysis of the Programs and Effectiveness of Peace Societies.* RALPH H. GUNDLACH, University of Washington.

The programs of about 30 peace and patriotic societies were examined and tabulated, and their answers to the SPSSI committee on war and peace questionnaire tabulated. The analysis converts the material into definite statements of policy upon topics of Foreign Affairs, Profits of War, Preparedness, Domestic Problems, and recommendation for Personal Action. From a survey of the data, it is possible to compare the various societies in terms of the composite program of all peace societies, and to rank them with regard to their extent and quality of action. It also becomes possible to evaluate what are considered to be the important and the unimportant items by the various societies, in preserving the peace, and to compare these recommended policies with the course of American policy. [10 min.]

10:55 A.M. *A Survey of Social Scientists' Opinions on Methods of Preventing War.* Ross Stagner, Dartmouth College, and J. F. Brown, University of Kansas.

In an attempt to reach a consensus of opinion among experts on controversial topics relating to war prevention, a questionnaire was sent to 375 prominent persons in the fields of history, economics, sociology and political science. For the most part persons who had done previous work on war were selected. A heavy majority were college teachers. One hundred and forty-two answers were received in form suitable for tabulation.

Quantitative results: greatest agreement was shown on such questions as opposition to economic nationalism and high tariffs (almost unanimous), education to understand foreign viewpoints and foreign ideologies were rated next; then opposition to fascist tendencies in this country and the strengthening of organizations such as the World Court. Proposals which were rated as possible *causes* of war were: national self-sufficiency; opposition to economic collectivism; Ludlow referendum; build world unity through the working class and Soviet Russia; maintaining trade with other countries during war; and increasing our military strength.

Qualitative results: some of the experts emphasized, both in their ratings and in free comments, the importance of political solutions (international organization); others favored psychological solutions (tolerance and education). In general, this bias followed the field of specialization of the individual.

Some subjects endorsed specific wars, while deprecating wars in general, *e.g.,* class war was abhorred by some, approved by others. Some "would rather fight than have a lot of x's here," referring to proposed relaxation of exclusion acts.

The presence of stereotyped reactions is indicated by divergences in answers to questions covering practically identical material, with and without stereotyped phrases.

The results are interpreted as indicating that decreases in economic, military, political and psychological nationalism would, in the opinion of these experts, work in the direction of war prevention. R. K. White and R. H. Gundlach assisted in the survey. [15 min.]

PHYSIOLOGICAL PSYCHOLOGY

Tuesday, September 5, 9:00 A.M.
Room 521, Hydraulic Laboratory
FRANKLIN FEARING, Chairman

9:00 A.M. *Recovery Sequence After Anesthetization with Ethyl Alcohol.* ALBERT C. CORNSWEET, University of North Carolina.

This study educed further evidence of caudo-cephalad induction and cephalo-caudad recovery upon anesthetization with gaseous ethyl alcohol. The behavioral patternings were of similar sequences noted in studies of developmental sequences. Thirty white rats were used of a wide age group range, twenty of which were subjected to the anesthetic effects of volatile absolute alcohol. Sequential patternings were observed to be of a clear order of appearance and disappearance in a fairly definite temporal sequence. The animals upon induction evinced behavioral tendencies of a caudo-cephalad nature while their recovery was cephalo-caudad. Any positivistic interpretation of these results would be too dogmatical. However, a probable explanation will be attempted. This theoretical explanation shall try to explain wherein the methodology and interpretations of previous studies have erred. [10 min.]

9:15 A.M. *The Effect of Partial Denervation of the Stomach upon Hunger in White Rats.* CLIFFORD T. MORGAN, Harvard University.

The present study seeks to determine whether the effect of insulin upon hunger strength, secured in an experiment previously reported, is related to gastric motility or to some other factor.

Fourteen albino rats were divided into two equal groups matched according to weight. One group was subjected to complete subdiaphragmatic section of the vagus nerve and partial section of the gastric splanchnic nerves. Such an operation eliminates any effect of insulin upon gastric contractions. After recovery from the operation, these and the normal rats were run daily for one hour in a Skinner-type feeding apparatus. In all 14 animals, insulin was injected on one day, an equal volume of physiological salt solution on the next day, and so on in alternation. Zero, 15, and 30 minute intervals between injection and time of feeding were used in separate experiments. Later, the sucrose content of the diet was reduced from 20 per cent to zero and part of the study repeated. In all, over 600 curves were secured, tabulated, and treated statistically.

The results show that with a high sucrose diet, insulin injection reliably increases eating in vagotomized rats. On this same diet, normal rats reduce their eating with insulin injection. Normal rats give the same reaction as vagotomized individuals, however, when sucrose is eliminated from the diet.

The results are taken to indicate that an increase of eating with insulin injection depends not upon any change in gastric motility but upon blood sugar level. They will be discussed in relation to the local theory of hunger.

This research was carried out at the University of Rochester in collaboration with J. D. Morgan. [15 min., slides.]

9:35 A.M. *Artificial Oestrus in the Guinea Pig Without Ovaries and Uterus.* GEORGE N. PAPANICOLAOU, Cornell University Medical College, and JOHN P. SEWARD, Connecticut College.

Previous investigators have induced receptivity in the spayed guinea pig by estrin-progesterone injections. Our problem is to determine whether these hormones act indirectly, through the remaining reproductive tissue, or directly on the central nervous system.

The method is as follows: A double ovariectomy and hysterectomy are performed. After recovery the animal is injected by Young's method and tested at intervals for the copulatory reflex.

To date, the experiment has been performed twice with one animal and once with a second. In all cases "heat" was successfully induced (see accompanying film). This indicates that the uterus is not essential to the reaction. The next step, now being attempted, is to remove the vagina as well, to see if oestrus can be induced in the absence of the entire reproductive tract. (Aided by the National Research Council Committee on Sex Problems.) [10 min., slides.]

9:50 A.M. *Temporal Relationships in the Copulatory Acts of Adult Male Rats During Three Hour Tests.* CALVIN P. STONE, Stanford University.

The gross descriptive features of a single copulatory act by male rats have been described by several investigators. No one, however, has made a systematic study of the temporal pattern of copulatory behavior leading to sexual non-aggression or satiation.

In male rats each ejaculation is preceded by a number of momentary intromissions (seldom less than 4 or 5). Following the ejaculation a pause that is variable as to duration from individual to individual but seldom less than 4 minutes long ensues. This pause gradually lengthens after the first ejaculation so that pauses of from 30 minutes to an hour or more may be expected during the third hour. Available data show that individual differences with respect to the temporal pattern of copulatory acts are more pronounced than those pertaining to the individual act and that both extrinsic and intrinsic factors contribute measurably to these individual differences. The measured effects of some of them will be exemplified. [15 min., slides.]

10:10 A.M. *The Effect of Sensory Stimulation on Heart Rate of Fetal, New-Born, and Adult Guinea-Pigs.* LEONARD CARMICHAEL, Tufts College.

In the present study, a suitable amplifying system and oscillograph have been used in the photographic recording of electrocardiograms of fetal, new-born, and adult guinea-pigs. In part of this study, an apparatus has been used which employs thyratron tubes tripped by the heart impulse, so arranged with condensers that a graphic record is given of change in rate of individual beats. In fetal animals, prepared according to techniques previously described, maintaining placental circulation, a new type of collodion-sealed electrode was used. Sixty-six animals have been studied with the following results: There is a variability between different animals in each age group, both in frequency of beat and in constancy of beat rhythm. Fetal heart rate, under the conditions of our experiment, is relatively slow; that of the new-born animal, rapid. Adult heart rate is intermediate between the two. The total range of all animals is from 50 to 393 beats per minute. Photic stimulation has no constant effect. Sound stimuli are often followed by a slowing of rate within a period up to ten seconds. This effect is less constant in fetal than in air-breathing animals. The heart component of the "startle pattern" shows a general ontogenetic development. It is, however, too variable an indicator to use in the study of growth of receptor capacity in the visual or auditory fields in the fetal organism.

Tables and sample records will be shown. The writer wishes to express his gratitude to Mr. Bertram Wellman for his assistance. [15 min., slides.]

10:30 A.M. *Influence of Pressure and Other Factors on the Polarity Potential of the Human Eye.* Walter R. Miles, Yale University.

A technique for recording the steady potential of the eye *in situ* and data on the reliability of this measurement have been recently published. The writer has also shown that this potential gives evidence of being influenced by fear or apprehension, as experienced in subjects who are not familiar with the measurement routine. The present report concerns preliminary experiments undertaken to explore the possibility of modifying the potential by means of eye-lid position, application of massage and of direct digital pressure, and from bathing the eyes in a hypertonic sodium chloride solution. A group of 18 young women who had served in previous eye-potential tests were used as subjects. In addition to confirming the earlier findings the present results indicate: (1) the potential as measured tends to be slightly higher when the eye is closed than when normally open; (2) the potential slowly decreases with repeated measurements in a relaxed posture and tends to come back up when the posture is changed; (3) more than half the subjects demonstrated a slight rise following 15 seconds of digital massage of one eye while the other (control) tended toward a lowered value; (4) direct digital pressure of moderate intensity raised the average value only slightly; (5) the introduction of 3 or 4 drops of 10% sodium chloride solution into the conjunctival sac of each eye produced a marked elevation (about 30%) and elevation lasted well beyond the time when any smarting or discomfiture were reported present. In general the results indicate that the polarity potential of the eye as measured by leads from adjoining skin areas is relatively stable but possible of artificial modification. [15 min., slides.]

10:50 A.M. *The Relation of Pilomotor, Pupillary, Cardiac and Respiratory Responses.* C. M. HARSH, J. G. BEEBE-CENTER, and S. S. STEVENS, Harvard University.

The association of cardiac acceleration and pupillary dilation with voluntary pilomotor erection of hair has been interpreted by Lindsley and Sassaman as indicative of generalized sympathetic discharge. Earlier investigations of the visceral concomitants of 'voluntary' breathing changes suggested to us, however, that the cardiac and pupillary responses might be functionally correlated with breathing changes alone.

By recording (in various combinations) heart rate, breathing, pilo-erection, and pupillary dilation it was found that pupillary dilation and cardiac acceleration can be elicited by voluntary breathing changes. Furthermore, pilo-erection can be elicited without cardiac acceleration, and *vice versa.* It is concluded that Lindsley and Sassaman's data are not proof of a generalized sympathetic discharge and, further, that the pilomotor response can be elicited without generalized sympathetic activity. [10 min., slides.]

SOCIAL FACTORS IN PERSONALITY

Tuesday, September 5, 1:30 P.M.
Room 2, Education Building
JOHN F. DASHIELL, Chairman

1:30 P.M. *Differences in Clique Formation Among Adolescent Boys and Girls Revealed by a "Guess Who" Technique.* HELEN M. CAMPBELL, University of California.

Distinctive sex differences in friendship groupings are shown by analysis of responses to the item "Here is someone who is my best friend" appearing on a personality inventory of the "Guess Who" type, given to members of a ninth grade class, which included 75 girls and 77 boys, with an average age of 14.5 years. Classmates were allowed to mention each other for the various items as often as they wished. Association into cliques is descriptive of the girls, as contrasted with the boys whose social relationships are not rigidly structured.

That this difference is also reflected in the way in which judgments are made for the other items of the Guess Who Test is revealed by a number of significant comparisons. Boys mention a proportionately larger number of different individuals; girls mention the same individuals more often. Girls' mentions are made proportionately more often for best friends, or other members of their friendship groups, as contrasted with boys' mentions. There tends to be a uniformity by clique members in their mentions outside the clique, for girls. Proportionately more unfavorable mentions are made by girls than by boys, for those outside the clique.

Correlations, critical ratios, and sociograms are presented. [15 min., slides.]

1:50 P.M. *The "Kid-Friend" Relationship: Some Observations on the Dynamics of Social Interaction.* SAMUEL B. CUMMINGS, JR., Kenyon College.

This study describes a complex system of intimate personal relationships that has arisen in an institution for mentally defective males. Typically, the relationship consists of a pairing of an older boy, the "friend," and a youngster, the "kid," who embark upon a rather exclusive and monogamous personal association which involves mutual loyalty, obligations and benefits. While somewhat labile and subject to dissolution when either participant becomes dissatisfied with the other, the association is rationalized, moralized and defended by resistance and resentment against external interference with its operation. It is freely and easily contracted and is not essentially a dominant-submissive form of association. In the larger setting of the institution, the participants tend to derive an enhancement rather than a loss of status and prestige from other patients. The function of the relationship in fulfilling individual needs in a highly simplified, uniform and depersonalized environment is described. The similarities of the relationship to established forms of personal association are also suggested. [15 min.]

2:10 P.M. *Personality Differences Between Boys and Girls in Early Childhood.* BRUNO KLOPFER, Rorschach Institute, New York City.

Preliminary experimental studies with the Shady Hill Growth Study, Harvard University, and with the experimental nursery schools of Vassar and Sarah Lawrence Colleges have demonstrated the applicability of the Rorschach method to personality diagnosis in early childhood. Matching and ranking experiments have established the reliability and validity of Rorschach interpretation at this age level.

As a by-product of these preliminary experiments, indications were found of sex differences at this stage of personality development. Further systematic investigations with children around the three, five, and seven year age levels, have shown the following results: (1) The girls, as a group, show reaction to external emotional stimuli in their responses at an earlier age (color responses), (2) the girls begin earlier to adjust their reactions to emotional situations (form-color combinations).

Out of the available material, the results of investigation with one hundred representative cases have been selected for presentation. [10 min., slides.]

2:25 P.M. *The Relationship of Hobbies to Personality Characteristics of School Children.* PAUL L. BOYNTON, Peabody College.

The study involves approximately 3,200 sixth grade children in 159 different schools, at least four of which were located in each of twenty-five states, distributed through practically all geographic regions. The data are taken from *The Coördinated Studies in Education*, the advisory committee of which is composed of the writer as chairman, and Drs. Greene of Iowa, King of Pennsylvania, McElhannon of Baylor, Obenchain of the Birmingham Public Schools, Otto of the W. K. Kellogg

Foundation, Segel of the United States Office of Education, and Van Wagenen of Minnesota.

From a series of data, the items of sex, age, scholastic aptitude, hobbies, teacher evaluations of personality, and personality test scores were selected for a Hollerith analysis. Among various problems investigated the following facts appear significant:

1. Certain hobbies, more than others, are associated with significant frequency with certain types of personality test scores. Sex differences are present in certain instances.
2. Teachers are likely to associate certain types of personality traits with certain hobbies with some greater frequency than is the case with other hobbies. Some evidences of sex differences appear.
3. The absence of a hobby is likely to be associated with a different type of personality score and different type of teacher evaluation from that which is found when certain hobbies, at least, are present.
4. The rôle of tested intelligence is not clear-cut in relationships noted.
5. The present data lend support to the frequently advanced hypothesis that hobbies may have a definite mental hygienic value with children. [15 min.]

2:45 P.M. *Levels of Aspiration in Academically Successful and Unsuccessful Children.* PAULINE SNEDDEN SEARS, Yale University.

Thirty-six children having widely divergent past experience of school success or failure were studied individually by standard clinical and level of aspiration procedures in order to determine (1) the relation of long-continued academic success or failure to level of aspiration pattern for school subject matter tasks, and (2) personality correlates of the level of aspiration pattern.

The results indicate that past experience of success or failure is significantly related to both size and variability of discrepancy scores (level of aspiration score minus performance score) on experimental materials closely resembling those on which the experience of success or failure had previously occurred. Past experience of success is found to be associated with a low positive discrepancy score and with lesser variability of this measure, while past experience of failure is associated with a high positive discrepancy score and with greater variability. Brief experimentally induced success and failure are found to have, in general, the same effects on the discrepancy scores as the past experience of long-continued success and failure, although these differences are not highly significant statistically.

Patterning of level of aspiration response, in terms of three measures of the functioning of the level of aspiration other than discrepancy, is not found to occur differentially in groups varying in past experience of success or failure. When the subjects are reclassified on the basis of size and consistency of discrepancy scores, however, clear patterns of level of aspiration response appear. This classification also brings out certain similarities in characteristic forms of reaction, in addition to the level of aspiration, in the personality structures of subjects classified together. These similarities reveal the presence of clusters of associated personality attributes, the characteristic level of aspiration response being itself one of these attributes. [15 min., slides.]

3:05 P.M. *The Relation of Certain Personality Variables to Level of Aspiration.* JOHN W. GARDNER, Connecticut College.

Earlier studies by Frank and by the present experimenter indicated that individual differences in aspiration-level in an experimental situation can be reliably measured and are sufficiently generalized to appear in dissimilar tasks. The present study attempts to test the assumption of previous investigators (Hoppe, Frank, Jucknat) that there is a direct relationship between aspiration-level behavior in an experimental situation and certain personality traits.

The subjects were 51 boys, high-school students, taken from a larger group which has been intensively studied in connection with the University Adolescent Study in Berkeley, California. Aspiration-level scores were obtained in connection with performance on a digit-symbol substitution test. False performance scores (identical for all subjects) were reported to the subject after each trial in the task, and he was then required to predict his performance on the ensuing trial. The aspiration-level score is based upon the extent to which his estimates of future performance deviate from his level of past performance. Ratings were obtained on eight personality variables, including all of those which Hoppe, Frank and Jucknat assumed to be directly related to aspiration-level behavior. The ratings were made by psychologists on the staff of the Institute of Child Welfare, all of them having had extended contact in a variety of situations with the subjects rated.

The correlations obtained between ratings and aspiration-level scores were for the most part insignificant. In a table of forty coefficients only two were four times as large as their probable error. It is concluded that there is no justification for assuming that a particular sort of aspiration-level behavior may be taken as unequivocal evidence concerning the presence or strength of any one of the traits dealt with. [15 min., slides.]

3:25 P.M. *A Study of Parental Dominance and Submission.* PERCIVAL M. SYMONDS, Teachers College, Columbia University.

Twenty-eight pairs representing respectively children who are much dominated by one or both parents and children whose parents are very submissive toward them were studied. The children in each pair are of the same sex, and roughly equated for age, school progress, intelligence, and socio-economic status. Children who are dominated by their parents have the better socialized behavior. They are polite, honest, careful, and dependable, but also, are sensitive, self-conscious, submissive, shy, and docile. Children of submissive parents are disobedient and irresponsible. They defy authority, and are unmanageable, but they are independent and self-confident. Each extreme, therefore, has positive and negative characteristics.

In their inner life the dominated children feel inferior and inadequate, are confused, bewildered, and inhibited. The children of submissive parents, on the other hand, feel self-important and over-confident, are given to boasting, and are considerably less inhibited.

In the case of the dominant parents it was found that usually one parent is in ascendance, dominating the whole family situation with a character which is compulsively neurotic. In the case of submissive parents there is frequently inadequate personality accompanied by childishness and failure to accept responsibility.

Dominant parents had dominant parents and submissive parents had submissive parents. An interesting reversal is seen here, for children of dominant parents tend to be submissive as children, but later, as parents, become dominant over their own children. [15 min.]

3:45 P.M. *Prestige and Psychometrics in a Primitive Society.* CECIL WILLIAM MANN, Claremont Colleges, Claremont, California.

Among the principal elements in the Fijian social and economic order are the acceptance of chieftainship, communalism and ceremonial. All three afford opportunities for the development and maintenance of prestige. Chiefliness is inherited, rank depending upon the degree of chiefliness of both father and mother. Rank carries with it the privilege of social superiority and power, and the responsibility of maintaining prestige. From chiefs of dependent tribes, personal and communal tribute may be exacted in the form of native labor and produce. A proportion of the latter will be perishable and will be redistributed in the form of a feast. The amount of tribute that can be demanded and the size of the feast will be measures of the prestige of a chief. The communal system is rooted in the borrowing of labor and goods. Only the most exceptional circumstances could impel a Fijian to reject a communal request. The number of persons who may borrow from him and from whom he may borrow will be criteria of the prestige of the Fijian. Ceremonial presentations are frequently made and enhance the prestige of the chiefs and the commoners. The chiefs gain prestige by the individual presentations which are made, while the strict order of drinking precedence preserves the prestige of the individuals. Some tribes have acquired prestige through the development of special abilities, *e.g.* the carpenter tribe and the firewalkers.

Administering a testing programme in Fiji, the writer had opportunities for noticing the effect of the above mentioned elements on testing. In some cases, the natives were greatly surprised to find that they were not allowed to assist each other in the group test. Many of them consequently lost interest. Other instances are cited to indicate the importance of controlling cultural variables when the results are to form the bases of race comparisons. [15 min.]

4:05 P.M. *Adulthood of the Favored Child among the Northern Blackfeet.* L. M. HANKS, JR., University of Illinois.

Among the competitive patterns of the north plains Indians, giving took a prominent place. A man increased his prestige through giving for large gifts still larger gifts in return. When gifts were made in the name of a child, most frequently a son, the man and particularly the child received special status which for the child was called the favored child or ***mĭnípuka.***

The status of ***mĭnípuka*** was acquired gradually through abundant giving. Ordinarily should any one make a small gift to a child, an equivalent return gift would be made. The *mĭnípuka* gave in return gifts many times the value of the original gift. As a result a *mĭnípuka* was accorded special treatment: He was sheltered from sordidness, not expected to work, provided with supernatural aid through many sacred objects, was entered into many societies, and at social gatherings was given the seat of honor and choicest food. After the death of the parent or upon reaching maturity, the *mĭnípuka* was expected to continue an ostentatious pattern through his own initiative and industriousness.

This shift from a semi-parasitical role to a role of providing and giving was accompanied by many difficulties. The individual had to assume adult responsibilities, for which he was ill-prepared. Frequently there is found a period of ostentatious giving followed by bankruptcy. A second pattern is miserliness which is equally reprehensible. In work many continue in slothful habits, thus losing prestige. In social groups some become shy and retiring while others are cranky and irrascible. Case studies are offered to substantiate these patterns. [15 min.]

HUMAN LEARNING, I

Tuesday, September 5, 1:30 P.M.
Auditorium, Education Building
EDWIN R. GUTHRIE, Chairman

1:30 P.M. *Retention and Transfer in Different Types of Training.* GEORGE KATONA, New School for Social Research, New York.

The curve of forgetting has been frequently studied with different types of material, but comparatively seldom with different types of learning methods, such as mechanical memorization, learning by understanding, and intermediate methods. The author in using such methods in acquiring knowledge of the same material, *e.g.* how to solve a difficult problem, obtained extensive differences in the rate of forgetting and the transfer effect.

Experiments were devised in which not only the learning method but also the form of the tests was varied. Two comparable groups which both learned to understand the essential points of a principle were tested in weekly intervals. To Group I the same problems were presented each week. A practice curve was obtained with regard to the specific problems which were tested often. But the performance became stereotyped and solving variations of the problem caused great difficulties. The final performance was similar to that of another group which had learned by memorizing specific solutions.

Group II was tested in weekly intervals with different problems (representing variations of a principle). A curve differing from the usual curve of forgetting was obtained. After an initial loss the performance improved slowly but steadily. The final accomplishment, measured by the ability to solve variations of the problem which were not taught or tested before, was the highest with Group II, then followed the control group (which was not tested at all in the interval), and was lowest with Group I.

The presentation of different examples, in the training or the testing period, may influence the performance favorably. That applies to a learning process which can be characterized as understanding of whole-qualities developed by improving the organization of the material. [15 min., slides.]

1:50 P.M. *A Spontaneous Recovery Curve for the Galvanic Skin Response in Human Subjects—A Discrepancy Between Results Obtained in Theoretically Parallel G. S. R. (Human) and Bar-pressing (Rat) Experiments.* DOUGLAS G. ELLSON, Stanford University.

The purpose of the experiment was to plot a curve of spontaneous recovery as a function of the recovery interval with human subjects, using the galvanic skin response. The training or conditioning procedure for four groups of 20 subjects was the conventional paired presentation of tone-shock combinations to establish a conditioned G. S. R. The response was then extinguished to a criterion (one-third of its original amplitude). The subjects were then given a rest interval of 5, 20, 60, or 180 minutes, a different interval for each group. Following the interval, the spontaneous recovery of the response was measured by a second extinction. Two measures of "strength of the response tendency" were used: (1) mean amplitude of the first two responses, and (2) number of responses required for extinction. The first measure presented a negatively accelerated curve of recovery similar to that found in a previous rat experiment with the bar-pressing response. The second measure showed no evidence of a progressive recovery—in fact there was an unreliable trend in the opposite direction.

An hypothesis is suggested to explain these results on the basis of two factors: (1) expectation of shock (affected by verbal evaluation of the situation), and (2) negative adaptation of the capacity to respond. [15 min., slides.]

2:10 P.M. *The Temporal Factor in Retroactive Inhibition.* EDWIN B. NEWMAN, Swarthmore College.

The more important studies of retroactive inhibition during recent years have laid major emphasis upon the factor of similarity and the concept of negative transfer. There are, however, several studies, notably those on reminiscence, which have appeared quite recently, indicating that the temporal factor in retroactive inhibition cannot be neglected. It seemed worth while, therefore, to repeat with much greater variety of temporal conditions the experiments of Robinson and McGeoch which yielded negative results. The procedure involved measuring retroactive inhibition with conventional memory materials, varying the total time between 24 hours and 14 days and the position of interpolation throughout the entire interval. At the present time it is possible to report certain preliminary results.

1. The finding of I. Miller that there is a sharp rise in inhibition as the material is brought into very close proximity with the reproduction has been completely confirmed.

2. The susceptibility of material to interference, measured either as an immediate effect or as an effect persisting over a period of time, decreases with increasing age of the original material.

Whatever may be the form of the final results demonstrating these conclusions, there can be no doubt that important and systematic changes do take place as a function of time. It is doubtful, however, that time itself should be considered the controlling variable. [15 min., slides.]

2:30 P.M. *Retroactive Inhibition Tested Immediately.* W. B. PILLSBURY, University of Michigan.

A study of so-called retroactive inhibition has dealt mostly with learning of series and with tests made at least a day after learning. This investigation was of the effect of work done immediately after learning upon memory span. The observers were shown eight geometrical forms or words, tachistoscopically, then ten seconds later were asked to write what they had seen. In the experimental series they were asked to spend the interval solving arithmetical problems or writing answers to questions. In the comparison series they did nothing. The results showed that both forms of distraction were effective in reducing the amount seen on the average of from twenty to more than fifty per cent. One subject, for example, reported an average of 4.6 seen with distraction and 3.7 without. Others showed larger differences. The differences are, in the main, statistically reliable.

When six nonsense syllables were offered serially with a memory apparatus, a similar reduction in the amount recalled after ten seconds with distraction as compared with ten seconds without distraction was found. The results indicate that part of the effect in retroaction may be due to mere interference with the setting of the material as well as to associative inhibition.

The writer is indebted to Ann Sylvester for assistance in the conduct of the experiment. [15 min.]

2:50 P.M. *The Specificity of Retroactive and Proactive Inhibition in Serial Verbal Habits.* ARTHUR W. MELTON, University of Missouri.

The purpose of this study, made in collaboration with Rebecca S. Walborn, was to determine the extent to which inter-serial inhibition is specific to the similar items in the two series. Two sets of 3-consonant "syllables" were used. The syllables in one set (X) were made from one group of 9 consonants and the syllables in the second set (Y) were made from a different group of 9 consonants. In the usual retroactive inhibition experiment, the original list was made by alternating the syllables from the X and Y groups, and the interpolated list was composed entirely of X syllables or entirely of Y syllables. In this way it was possible to determine the extent to which the interpolated learning interfered with the recall and relearning of the similar and dissimilar items in the original list. The experiment was repeated with the original and interpolated lists reversed, in order to determine in the same way the specificity of proactive inhibition in the learning and in the recall and relearning of the second list. Forty-eight S's were used, and the factors of practice, serial position of similar and dissimilar items, and difficulty of specific lists were equalized for the various conditions.

Results: The earlier finding that retroactive inhibition is significantly greater than proactive inhibition, when measured by recall and relearning scores, was confirmed. Furthermore, both proactive inhibition and retroactive inhibition were found to be significantly greater in the recall and relearning of the similar items in the two lists than in the recall and relearning of the dissimilar items. There was, however, some inhibition of the dissimilar items. Specific inhibition was also found during the learning of the second list. Implications for theories of retroactive inhibition will be considered. [15 min., slides.]

3:10 P.M. *Remote Associations as a Function of Interpolated Learning.* JOHN A. MCGEOCH, State University of Iowa.

During the learning of serial lists associations are formed not only between adjacent items in a forward direction, but also between certain items and others removed from them by one or more skipped terms in either the backward or the forward direction. These remote associations represent an organization of the list whereby it is knit into a much greater unity than the usual method of serial testing would imply.

The present experiment has studied the influence of interpolated learning upon these remote associations, when they are elicited by a modified free association technique. After practice with the method, seventy college men went through two rest and two work conditions in a counterbalanced order. The learning materials were lists of 10 adjectives, and the interpolated lists were synonyms of those in the original list. The test for remote association began ten minutes after the completion of five learning repetitions. Associative latency was measured by a microphone-voice-key-graphic-recorder setup.

A large number of remote associations were formed, and the interpolated learning produced a substantial decrement among them. This decrement was not as large as the decrement among the immediately adjacent associations. It was equal for remote backward and remote forward associations. These results contradict the expectations from results on total lists that the greatest decrement should be among the supposedly weaker associations.

The influence of interpolated learning is, thus, to disrupt the indirectly established organization of the list, as well as the direct serial associations, and to disrupt them in a manner which is independent of the supposed strength of the remote associations. [15 min., slides.]

3:30 P.M. *Memory for Physically Identical Elements in Human Maze Learning: A Transfer Problem.* WILLIS D. ELLIS, University of Arizona.

Transfer of training from one learning situation to another is commonly assumed to rest upon some factor of resemblance, similarity or, indeed, identity between the earlier task and a later one. However, Kurt

Gottschaldt's study of past experience has revealed that a familiar figure appearing in a new setting will sometimes not be seen at all (principle of camouflage) and that past experience is therefore under such circumstances of little or no importance. Similarly, Joseph Ternus has shown in cases of perceived movement that physical identity between two forms will or will not be experienced as *phenomenal* identity depending on structural features of the forms themselves, conditions of observation (distance, brightness), and so on. As regards problems of transfer in learning, a similar reinterpretation may be necessary, for it is perhaps doubtful if physical identity necessarily assures phenomenal identity.

As a device for investigating this, three finger mazes were used with 30 college students acting as subjects. Ten subjects learned Maze I with the right hand; 10 learned Maze II with the left hand. Maze III was a "dual" maze composed of Mazes I and II together on a single board, and this also was learned by 10 subjects. The preceding 20 subjects were then given Maze III to "relearn" although 10 of them already knew one-half of it and 10 the other half. Since there was physical identity between each of the preceding mazes and one-half of Maze III, a considerable transfer effect might have been expected. Actually the subjects who already "knew" half of Maze III had almost exactly as much difficulty in *learning* it as did the 10 control subjects who learned this maze without previous training. The experimental work reported in this paper was done in collaboration with Leslie Collie, graduate student in psychology at the University of Arizona. [15 min., slides.]

3:50 P.M. *The Influence of Goal-Distance and Route-Security upon the Velocity of Directed Action.* HERBERT F. WRIGHT, Carleton College.

The purpose of the study was to measure the psychological forces governing a course of directed action in relation to changes in *goal-distance* (length of *path* between individual and goal) and *route-security* ("safety" of path). The velocity of directed action was used as a criterion for the strength of a resultant of forces. Children were used as subjects.

The central feature of the experimental situation was a long "table." At one end of the table a lever was mounted. By operating the lever it was possible for the child to procure a marble which had been placed at the opposite end of the table. Three *path-structures* were provided: (A) secure (*i.e.* "broad and safe" throughout); (B) secure-insecure (*i.e.* broad and safe except for the middlemost one-third which was "narrow and hazardous"); (C) insecure (*i.e.* narrow and hazardous throughout). Each child participated in a series of at least ten trials involving one or, successively, two of the three path-structures. The velocity with which the child actuated the lever was recorded automatically in relation to the position of the marble on the table.

The results include the following. (1) All three paths: (a) an increase in velocity from trial to trial; (b) a marked retardation in the terminal sector. (2) The secure path: a virtually constant velocity except for the first trial on which there occurred a "slow start." (3)

The insecure path: an increase in velocity on all trials with approach to the goal. (4) The secure-insecure path: (a) a retardation in the "zone of insecurity"); (b) a relatively low velocity in the "zone of approach to the zone of insecurity." An attempt will be made to exemplify by a single analysis the field-theoretical treatment which has been made of these results. The research reported was conducted at the State University of Iowa. [15 min., slides.]

PERSONNEL

Tuesday, September 5, 1:30 P.M.
Room 521, Hydraulic Laboratory
HAROLD R. CROSLAND, Chairman

1:30 P.M. *An Application of the Life History Method.* MARY C. VAN TUYL, University of Michigan.

Description of a technique developed from the study of nearly 1,000 autobiographies of college students, collected during a period of 8 years. These autobiographies were guided by outlines given to the students and therefore they contained comparable material on background and on present thinking.

In order to handle this complex material a code list was constructed of 150 items containing from 2 to 66 classes under these items. This code formed a framework into which a large number of details from each autobiography was placed and transcribed to Hollerith cards. The key item of the study was the one in which these students were distributed into classes according to their present religious mode of thinking, ranging from extreme orthodoxy to four classes of modern liberals. Relation of position in this classification to a number of other items will be discussed. [10 min., slides.]

1:45 P.M. *Predictive Value of College Admissions Data.* MARGARET MERCER, New York State College of Home Economics, Cornell University.

This study is preliminary to a more complete inquiry into factors predicting success in the N. Y. State College of Home Economics. The investigation is made to help solve problems arising from the need to evaluate credentials of six times as many girls as may be accepted.

Data studied consist of regents' grades, principals' personality ratings, high school activities, and rating on a personal interview. Subjects were: members of the three upper classes who for any reason had left college without receiving a degree; equal numbers of students who stood highest and lowest respectively in the same classes.

Consistent differences are shown in high school grades and principals' ratings in favor of the group now ranking highest in college, but not in high school activities or in ratings on personal interview. The findings are supplemented by case studies indicating influence of personal adjustment factors. [10 min.]

2:00 P.M. *Corrective Reading in College.* BERT A. NASH, University of Kansas.

A semester experiment in the improvement of the reading ability of 121 college students, through the coöperation of graduate students enrolled in a course in Reading and Study Laboratory. A modified tutorial system was employed, supplemented by the use of various instruments now used experimentally in many clinics. The data include analyses of the types of reading difficulties of college students, their opinions of their difficulties, and a description of the individual corrective measures used.

The results of the experiment indicate that 86% of the students made significant improvement in all aspects of the reading process, as measured by batteries of standardized tests and photographs of eye movements. Data will be presented also to show the permanence of this improvement after an interval of one year. The group data will be supplemented by case studies involving particular reading difficulties and remedial measures. [10 min.]

2:15 P.M. *Significance of the Point of Reference.* EDWARD K. STRONG, JR., Stanford University.

The interests of physicians correlate .16 or .83 with the interests of lawyers: similarly the interests of physicians correlate —.51 or .49 with the interests of life insurance men. (In both cases the test blanks of identically the same samplings are used.) Almost any other correlation may be obtained using these test blanks.

When professional men are contrasted with one another it appears that the interests of physicians differ greatly from lawyers (.16). But when all occupations are considered the interests of physicians and lawyers appear to be nearly alike (*r* of .83).

The relationship between the interests of any two occupations is relative, not absolute. That relationship depends among other things upon the point of reference. When the point of reference is centered among the occupations to be considered large plus and minus correlations result: when the point of reference is outside the group of occupations, only plus correlations result: the farther removed is the point of reference the higher must these plus correlations be. [15 min.]

2:35 P.M. *Primary Ability Tests Applied to Engineering Freshmen.* ROBERT G. BERNREUTER, Pennsylvania State College.

Thurstone's Primary Ability Tests, Experimental Edition, were administered to 170 engineering freshmen at The Pennsylvania State College. The reliabilities of the sub tests ranged from .75 to .96 with a median of .93. The primary ability intercorrelations ranged from .03 to .45 with a median of .24.

Engineering students were found to be significantly different from the high school seniors reported by Thurstone in that they scored higher in the spacial and reasoning factors and scored lower in the verbal and and induction factors.

The primary abilities were also correlated with scholastic success in mathematics, English, drawing, chemistry, and psychology courses. On the basis of the results, it seems reasonable to expect to find significant relationships between the primary abilities and success in other specific courses and curricula.

The study was conducted with the coöperation of Mr. Charles H. Goodman. [10 min.]

2:50 P.M. *A Study of Employees' Attitudes.* HAROLD B. BERGEN and JOEL DEAN, McKinsey, Wellington & Company, New York.

This study was made to determine the relative morale of groups of industrial and office workers and their attitudes towards specific personnel practices. More than 1,000 employees of one company checked the items with which they agreed in a questionnaire consisting of attitude scale statements and multiple-choice questions.

The average morale scores varied from 6.94 in the highest department to 4.59 in the lowest, with an average score of 5.71 for all employees. The score of female employees with over 7 years' service, employed in the general offices of the company, was especially low in comparison with that of women with shorter service. Male employees with from 2 to 7 years' service had a lower score than those with greater or less service. Differences in supervision and attitudes towards the fairness of pay differentials were found to be the most important determinants of morale. [10 min.]

HUMAN LEARNING, II

Wednesday, September 6, 9:00 A.M.
Room 2, Education Building
WALTER B. PILLSBURY, Chairman

9:00 A.M. *Predicting a Learning Ability After Varying Intervals.* HAROLD D. CARTER, Institute of Child Welfare, University of California.

A paired associates learning test was administered repeatedly to about fifty boys and fifty girls, at six-month intervals for four years beginning at age twelve. The task was to recall words in response to pictures. The words and pictures were completely different at each successive testing.

The reliability coefficients obtained from odd and even halves averaged .86. Inter-correlations between successive tests declined from an average of .64 for a six-month interval to .41 for a three-year interval. The correlations increased again for intervals of 3.5 and 4 years.

Studies in which intelligence tests have been repeatedly administered have shown higher re-test correlations for short intervals, and lower ones for long intervals. The comparative content of the tasks suggests one explanation. Consideration of relationships with periods of rapid growth suggests further explanations. [10 min., slides.]

9:15 A.M. *Negative vs. Positive Practice in Correcting Spelling Errors.* HELEN PEAK, Randolph-Macon Woman's College.

Forty-eight college students were scored on their spelling of a list of 120 words chosen from a standardized spelling scale. One week later a second test was given. The subjects were then divided into two groups equated for scores on American Council Psychological tests, college class, average difficulty and number of words missed on the tests. The positive group wrote correctly five of their misspelled words 32 times, five, 16 times. The negative group wrote five words as misspelled 32 times, five, 16 times, with instructions to remember that the spelling was incorrect. The difficulty of the words practised was also equated for the groups. Three days later both original forms of the test were given. Three weeks later, both tests were given again. Only small differences appeared between the groups with slightly fewer spelling errors for the positive group. [10 min., slides.]

9:30 A.M. *A Study of the Developmental Aspects of Child Thought by a Clinical Method.* BYRON C. SARVIS, University of Kansas.

Two hundred seventy-four children ranging in age from two to seventeen years were studied by a clinical method to inquire into the nature of the development of concepts of physical causality and moral judgment in the same child. Concepts of physical causality were obtained from reactions to a series of simple demonstrations of buoyance. Concepts of moral judgment were elicited by participation in and explanation of social games. Piaget's seventeen types of thinking served as a point of departure for classification and interpretation of the data. The relation between the development of these two aspects of thinking and chronological age, mental age, and environment was also studied. The results show that there are not clearly differentiated qualitative levels characteristic of certain ages, although there are stages at which certain quantitative differences are found. [10 min.]

9:45 A.M. *Concept Formation in Children.* LOUIS LONG, Columbia Medical Center.

The aim of this investigation is to discover how and when a child recognized that certain characteristics of objects of the genus group are similar to those possessed by familiar species; to discover by what process the delimitation of the species name takes place; and to study the course of development of further hierarchies, their flexibility and their range.

This paper will be limited to a discussion of the concept of roundness in a group of fifteen children ranging in age from two to six years. A non-verbal discrimination technique has been developed which permits a careful control of stimuli and experimental conditions.

The stimuli were divided into three types: spherical objects, cylindrical objects, and two dimensional figures.

Twelve subjects established a concept of spherical roundness. In ten of these subjects the concept was broad enough to include cylindrical objects as well as spherical objects; and, similarly, the concept of spherical-circular roundness included two dimensional roundness and the rounder figure of a pair of non-round two dimensional figures. The spread of the concept of spherical roundness to the other types of stimuli was, however, not equal. The transfer was greatest from spherical to circular and least from spherical to the rounder of two dimensional figures.

An attempt has been made to isolate in the field of human development some of those factors determining range of equivalent stimuli which have been pointed out by Lashley and Klüver in their work with animals. [15 min., slides.]

10:05 A.M. *The Effect of Nursery School Attendance upon the I.Q.* James D. Page, University of Rochester.

Seventy-five children who had attended nursery schools for a period of from one to three academic years were given the L form of the Stanford-Binet and their intelligence test scores were compared with similar test scores earned by their siblings who had not attended nursery schools. The interval between nursery school attendance and test administration ranged from a few months to four years. At the time of testing the median age of the nursery school group was 7 and that of the sibling control was 10.

Our data indicate that nursery school attendance does not increase subsequent intelligence test performance. The I.Q. of children who had attended nursery schools did not differ significantly from those of their brothers and sisters who had not attended nursery schools. [10 min., slides.]

AESTHETICS

Wednesday, September 6, 9:00 A.M.
Room 521, Hydraulic Laboratory
Madison Bentley, Chairman

9:00 A.M. *The Validity of Four Accepted Criteria of Design.* Llewellyn N. Wiley, University of Illinois.

Can a relatively unselected group of persons agree upon the pleasingness of a set of abstract designs? Will a group of artists agree as to which designs have the most symmetry, balance, rhythm, and unity, when the estimations of these qualities depend upon the artists' individual definitions? Will designs having the greatest amounts of symmetry, balance, rhythm, and unity be the most pleasing to the unselected group?

Fifty black-white, relatively abstract line drawings were rendered in uniform lantern slides and presented twice to 144 students of psychology 1, and 4 times to 5 members of the Fine Arts School faculty of the University of Illinois. Ratings on each of the designs in terms of "pleasantness" were obtained from the students, while ratings on symmetry, balance, rhythm, and unity were had from the specialists. If each design be given a score made up of the ratings it received from 72 of the 144 raters, the correlations between the 50 pairs of summed ratings prove to be .98 (first presentation) and .97 (second). The comparable group retest reliability is .94. Individual retest consistency was found for 138 subjects to have a median of .50. Ratings for "pleasantness" are therefore sufficiently reliable to demonstrate agreement among individuals. Estimation of symmetry, balance, rhythm, and unity have the following self-correlations, respectively: .90; .83; .87; .78 (three raters against two raters stepped up to a population 1.5 times that size). A correlation of nearly .90 between symmetry and balance suggests that the two must exist concurrently. The other measures are all positively correlated to a less extent. The correlation between the sums of the "pleasantness" ratings and the 4 criteria is .56 (first test) and .65 (retest). [15 min., slides.]

9:20 A.M. *The Effects of Protracted Observation of a Painting.* GILBERT BRIGHOUSE, Occidental College.

Fourteen paintings were closely examined by twenty-one observers for a total of ninety-six minutes each painting. Each observation period was four minutes for each painting, repeated four times weekly for six weeks. Each observer made rank-order comparisons of the paintings, and rated their hedonic tones on a seven-point scale, both before and after the experiment. It was found that paintings by the moderns (Modigliani, Cezanne and Van Gogh) gained considerably in hedonic tone, that the classic and romantic paintings gained slightly, and that losses were recorded for the extreme abstractionists (Duchamp and Picasso). The results have significance for a genetic theory of aesthetic experience. [10 min., slides.]

9:35 A.M. *The Effect of Register and Tonality upon Musical Mood.* MELVIN G. RIGG, Oklahoma A. and M. College.

Five musical phrases were played, each in five different transpositions, to eighty-four observers, who judged the emotional significance by means of a check list. In all, 2,100 judgments were made.

Transposing a phrase up an octave always made it happier, while transposing it down an octave had the opposite effect. For changes of approximately half an octave (transposition to the key of the dominant) the same rule held in eight cases out of ten.

By transposing the phrases up one-half step or down one full step, large changes were produced in tonality or key, although the changes in register were slight. No evidence was found in support of a view widely held among musicians that each tonality possesses a distinctive character. [10 min.]

9:50 A.M. *The Adlerian Principle in the Arts.* PAUL R. FARNSWORTH, Stanford University.

Two experimental attacks have been made on the problem of overcompensation in the arts. In one, children from the third through the sixth grade were rated by their teachers for art ability and were tested for color weakness. A slight positive correlation was found between excellence in color vision and art ability. The Adlerian hypothesis was not favored by these data. In a second experiment, auditory acuity scores of unmusical school children of the lowest four grades were contrasted with scores made by the most musical. The majority of critical ratios, while insignificant in size, favored the Adlerian contention.

In a continuation of this latter study, college students have been tested for auditory acuity, have taken the six Seashore music tests, and have rated their musical status on a five point scale. The resulting data do not favor the Adlerian theory. [10 min.]

10:05 A.M. *The Dynamic Quality of Perception.* HERBERT S. LANGFELD, Princeton University.

According to the motor theory perception is related to the muscular response of the observer. If this theory is true, then there is in all "external" perception a projection of response into the object. This statement would seem to be identical with the usual definition of empathy, and empathy would thus be an almost universal phenomenon. When concepts cover too large a range of facts, they are apt to lose their usefulness. It seems, therefore, more profitable to limit empathy to that projection of response which gives a dynamic quality to the objects of perception. The relation of empathy to aesthetic experience and its place in a psychological theory of beauty will also be discussed. [10 min.]

10:20 A.M. *Perceptual Characteristics of Schematized Human Figures.* EGON BRUNSWIK, University of California.

Twelve variations of a graphic, crudely schematized human figure, about half of them involving changes of facial appearance besides those of stature, were presented to 58 students, using the method of paired comparison. Among the six apparent characteristics tested, greatest agreement among the subjects was found for "good-lookingness," followed in declining order by "age," "energy," "likeability," "happiness," "intelligence." In approximately the same order there is an increase in the relative influence of the face, although even for the last two of these qualities, apparent happiness and apparent intelligence, significant differences can be found for pairs differing only in stature and not in facial proportion. Besides the general tendency to perceive as more intelligent the standard medium figure, men seem to rate athletic more intelligent than leptosomatic figures with little emphasis on height, women tall more intelligent than short figures with little emphasis on breadth. For such qualities as happy, good-looking, and energetic, however, women seem to be favorably impressed also by breadth. An example of the tendency toward ambivalent effects is shown by the addition of spectacles to the standard face which increases apparent intelligence and decreases good-lookingness of the figure. [15 min., slides.]

SOCIAL PSYCHOLOGY

Wednesday, September 6, 9:00 A.M.
Auditorium, Education Building
GOODWIN WATSON, Chairman

9:00 A.M. *An Experiment in Social Behavior.* L. J. O'ROURKE, United States Civil Service Commission.

An extensive research study involving some eighty thousand pupils was carried out in public schools.

Certain challenging questions arose: Can children assume the leadership in improving their social behavior? Can children set up essential criteria for their own social behavior? Can such criteria be evolved out of their own experiences? How can we motivate children to make continuing efforts to improve their own social behavior?

One approach used is based on the desire for approval by one's associates. We applied the psychological principle that a person has a special interest in something he has helped to create. Standards deduced from one's own experiences are more meaningful and more likely to be followed.

Children worked in committees to make lists of likes and dislikes illustrated by stories of their own experiences. The essentially desirable qualities of honesty, truthfulness, coöperativeness, responsibility, kindness, good sportsmanship, unselfishness, self-control, are all included in the children's lists, but are expressed in terms of specific actions drawn from actual experiences.

Pupils' stories, thousands of which were submitted, are evidence that pupils can evolve desirable standards out of their own experiences. Teachers' reports furnish evidence of positive effect the program had upon pupils participating.

Another especially effective approach, used at a higher level, is, briefly: When the pupil is tempted to do or say something unkind, he asks himself, "Would I do or say this if someone offered me a dollar to do it?" If his answer is "No," he says, "Why, then, should I do it for nothing?"

Effectiveness of this approach is evidenced by the stories of experiences in applying it. Such stories evidence the feeling of satisfaction that comes with achievement. We give the pupil the tool, but he applies it himself. [15 min.]

9:20 A.M. *The J-curve Hypothesis and Behavior in a Small Community.* R. H. WATERS, University of Arkansas.

Allport has suggested that the J-curve will fit behavioral data only when the social conforming agencies have a relatively high potency. This suggestion has been tested and found valid in two investigations in a small community. One study showed that, in this particular community, motorists' behavior toward a traffic regulation does not yield a J-curve. In the second study the time of arrival at work by employees in an industrial concern did conform to the J-curve. It is thus suggested that the J-curve, or the nature of the distribution of social behavior, may be taken as a measure of the strength of the social conforming agencies present. [10 min., slides.]

9:35 A.M. *Conforming Behavior and the J-curve Hypothesis.* FRANKLIN FEARING, University of California at Los Angeles.

The J-curve hypothesis of conforming behavior as presented by Allport and confirmed by Frederickson, Frank and Freeman is the subject of the present study. 1,541 observations were made on the behavior of motorists in a traffic situation in which conformity was primarily induced by stop signs, rather than by personal risk involved in non-conformity. Positions on a telic continuum were defined as follows: Stopped at line on pavement, stopped past line, slowed to 1 to 3 m.p.h., slowed to 3 to 6 m.p.h., slowed to 6 plus m.p.h., no change in speed. The data were analyzed in light of the following variables: (1) sex of driver, (2) size of vehicle, (3) number of passengers, (4) time of day, (5) day of week.

The results, when plotted, do not appear to confirm the J-curve hypothesis of Allport, but rather approximate a normal distribution curve.

It is suggested that the discrepancy between these results and those of other investigators is due to (1) the exclusion from our study of those cases in which the incentive to conformity included the possibility of a collision, (2) the use of chronometric methods in defining the positions on the telic continuum.

The use of "conformity" and "non-conformity" as descriptive categories in psychological analysis is discussed with particular reference to the distinction between objective or legal conformity and conformity in terms of the degree to which a given act satisfies the requirements of a specific social situation.

The relationship of conformity in the latter sense to various types of social situations is discussed.

Mr. E. M. Krise collected the data and assisted in the preparation of this paper. [15 min., slides.]

9:55 A.M. *The Construction and Use of a Telic Scale for Measuring War-producing Behaviors of Citizens.* FLOYD H. ALLPORT, Syracuse University.

The problem of international disputes may be restated as a societal event system, or field of human behaviors, from which war events emerge. This study deals with the significance of the acts of ordinary citizens within a country in an event system of this character. The principle of the telic continuum is employed, and psychophysical scales are constructed on the three variables of interest, effort and effect. Five types of international crises were employed as hypothetical situations for evoking the self-admitted responses to be measured. The scales are checked twice, once for individuals' own "performance," and once for their belief in what is right for citizens to do (*i.e.* their "consent"). The correlations between all these variables are high. "Consent" permits behavior a little more in the direction of war than does "performance." Scores on interest and effort run higher than those on effect. The stimulus situations used gave low mean scores and a narrow range, indicating the relatively slight role of ordinary citizens in war production. There were, however, reliable differences between the appeals. Their strengths for evoking war-producing behaviors fall in the following order: threat to political autonomy, threat to our war strength and armaments, threat to our own territory, threat to foreign possessions. Threat to national honor and rights abroad has an indeterminate position. The significance for these findings for official action and for understanding war-production is discussed. There is acknowledged the extensive work on scale construction and the statistical treatment done by Gertrude A. Hanchett. [15 min., slides.]

10:15 A.M. *A Field Study of Psychological Aspects of Acculturation.* IRVIN L. CHILD, Yale University.

This study presents an analysis of psychological aspects of membership in an acculturating group, based on a field study of second-generation Italian men in a New England city. The method of study was for the investigator to establish personal contact with second-generation Italians and obtain data by the use of four procedures: participant observation, life-history interviews with three persons, informal interviews, and standardized interviews with fifty informants.

Differences between Italian and American culture, institutions effective in producing cultural stability or change, and the status of Italians in the community are identifiable features of a social setting which poses psychological problems of adjustment for the second-generation individual. These problems may be reduced, in large part, to a conflict between acting so as to make himself acceptable to the Italian group and acting so as to make himself acceptable to the American group. The diverse modes of solution of this conflict are classified into three general types of reaction. In the "apathetic" reaction, the individual partially escapes from the conflict by a reduction of the instigation to pursue either of the conflicting sequences of behavior, and he does not become consistently affiliated with either nationality group. In the "in-group" reaction, the instigation to become accepted by Italians gains dominance, and the individual accepts membership in the Italian group. In the "rebel" reaction, the instigation to become accepted by Americans gains dominance, and the individual rebels against his membership in the Italian group and tries to be known as an American. The three types of reaction are described and interpreted as they are shown in individuals' relationships to their personal associates, to the nationality groups, and to culture traits. [15 min.]

10:35 A.M. *The Clubhouse Project in the California Adolescent Study.* W. J. CAMERON, Institute of Child Welfare, University of California.

For a period of three years a clubhouse adjoining the public school attended by members of the Adolescent Study was operated as a recreational center and for the observation of their leisure time behavior. A staff offered opportunities for participation in such activities as photography, carpentry, art, and dramatics.

Classes of data collected in the clubhouse situation include: (1) records of attendance and activities; (2) ratings of behavior (a) in the company of the same sex, and (b) in mixed groups; (3) descriptive records of evening parties as well as of day-to-day social relationships; (4) conversations of small groups; (5) products of art and dramatic groups; (6) estimates of reputation and expressions of social attitudes as recorded by the boys and girls themselves.

The study of these data in conjunction with other sources of information about the same population led to the following conclusions: (1) Social activities of mixed groups rose to a position of leading importance for the majority of this group of adolescents. (2) Individual differences in preferred activities bore significant relationships to factors of physique, appearance, and the degree of physical maturity. They were also related to athletic achievement, established attitudes toward parents and sibs, sociability, and many other behavior characteristics. (3) Sex differences in social maturity were related to the characteristic lag of boys in onset of the growth spurt behind girls of the same age. (4) The motivation underlying the choices of activities by boys and girls was principally directed toward the achievement of social status, or of acceptability to a social group. [15 min., slides.]

10:55 A.M. *Individual Differences as Related to Autocratic and Democratic Group Atmospheres.* RONALD LIPPITT and RALPH K. WHITE, Cornell University.

This is a second report on the program of research in group atmospheres which was begun by Lippitt in 1937 and continued by Lippitt and White in 1938, under the direction of Professor Kurt Lewin at the Iowa Child Welfare Research Station. A preliminary report last year stressed the nature of the experimental controls, and three generalizations about the atmospheres themselves: (1) Autocracy tended to bring out one or the other of two dissimilar patterns of behavior, apathy and aggression; (2) "laissez-faire," in which there was a minimum of adult participation, proved decidedly different from democracy; (3) friendly behavior was most frequent in democracy, hostile behavior in laissez-faire and in the aggressive reaction to autocracy. Much of this aggression was directed against scapegoats.

Further analysis of the data has been concerned especially with individual differences. One conclusion, for example, is that, in groups characterized by the apathetic reaction to autocracy, individual differences in behavior tend to disappear. Individuals tend to be "run in the same mold." Quantitative data have also been obtained on the behavior of each adult leader, showing how he extended or limited the children's psychological "space of free movement"; the behavior of the boys before the leader arrives or when he leaves the room; reactions to hostile strangers; factors associated with the outbreak of intergroup hostility; analysis of the data in terms of psychologically meaningful time units (using in modified form a technique developed by Barker and Dembo); analyses of stenographic records in terms of 124 psychologically-defined categories; reliability data; correlations with Rorschach data; three interviews with each child; interviews with teachers and parents, giving clues as to the historical origin of individual differences in reaction to authority. [15 min., slides.]

SENSORY FUNCTIONS

Wednesday, September 6, 1:30 P.M.
Auditorium, Education Building
HERBERT S. LANGFELD, Chairman

1:30 P.M. *The Importance of Impedence-Matching in Studies of Vibro-Tactile Sensitivity.* LOUIS D. GOODFELLOW, Northwestern University.

Recent studies show that many of the conflicting data reported in the literature of vibro-tactile sensitivity can be reconciled by an analysis of the actual experimental conditions. The age-old argument between those experimentalists whose data limits the perception of vibration to the skin and those whose results link the perception of vibration with deep sensitivity takes on a new aspect in the light of recent research which indicates that both deep and superficial sensitivity are involved, the relative amounts being a function of the characteristics of the vibrator.

The chief source of the conflicting results reported in this field appears to be in the almost universal assumption that the vibration applied to the skin is the actual stimulus. Studies on the physical characteristics of the vibrator indicate that the particular end-organs stimulated and the amount of energy transmitted to them varies for different types of vibrators. The match in mechanical impedence between the skin and the vibrator is important. For heavy well-matched vibrators, deep sensitivity is primarily involved in the perception of vibration, but with small poorly matched vibrators such as have been frequently used in the past, deep sensitivity is of little importance. Consequently, psycho-physical data on such topics as threshold values, adaptation, fatigue, frequency characteristics, and discrimination depend on the characteristics of the vibrator, which determine the relative amounts of deep and superficial sensitivity involved, and the amount of stimulation reaching them. [15 min.]

1:50 P.M. *A Possible Receptor of Cutaneous Pressure and Vibration.* B. VON HALLER GILMER, Carnegie Institute of Technology.

The present investigation has been an attempt to determine the possible sense organs of pressure and vibration by means of cutaneous excision. Recent experimental evidence has shown that points on the skin most sensitive to pressure stimulation are likewise very sensitive to vibratory stimuli, induced both mechanically and by the use of alternating electrical currents. " Pressure-vibration spots " on several regions of the back and arms were removed surgically and an histological examination of the underlying tissue was made. The histological analyses of the biopsies revealed what is believed to be glomus bodies. Control biopsies of spots insensitive to pressure and vibration did not contain the glomus organ. The possible functions of the glomus body as a receptor for cutaneous pressure and vibration are discussed in light of the present results. [15 min., slides.]

2:10 P.M. *Summation With Weak Visual Stimuli.* WARREN W. WILCOX, Albion College.

In this experiment, the size of the two stimulus patches was held constant and their separation varied. Summation thresholds were determined by reducing the illumination until the patches appeared homogeneous with the background.

With stimulus patches either lighter or darker than the background, the summative effect was found to cover between 30 and 40 minutes of visual angle within the fovea. Lighted stimulus patches showed less summative effect than did dark patches on a lighter ground. Summation decreased with the decrease in size of the stimulus patches and with an increase in the level of illumination.

The results indicate that the summative effect is due to the configuration of the stimulus rather than to the distribution of the anatomical units of the eye, since with the same illumination the summation threshold has widely different values. [10 min.]

2:25 P.M. *The Effects of Low Oxygen Tension and Low Blood Sugar on Dark Adaptation.* R. A. McFARLAND and W. H. FORBES, Harvard University.

Numerous experiments have demonstrated that the efficiency of sensory and mental functions depends upon the amount of oxygen being delivered to the nervous tissue. These studies have contributed to an understanding of the fundamental nature of psycho-physiological mechanisms. The study of dark adaptation is important, both from the point of view of the practical implications in night driving and in aviation and from the point of view of understanding the functioning of the central nervous system.

The thresholds relating to dark adaptation were made with the Hecht adaptometer. The experiments in low oxygen were carried out in a chamber in which the partial pressure of oxygen was diminished with nitrogen. The blood sugar was reduced by the subcutaneous injection of from 5 to 10 units of insulin, the exact amount being administered in relation to body weight. In the low oxygen series the measurement of dark adaptation was carried out at simulated altitudes varying from 11,000 to 20,000 feet. At the higher altitudes, while the subjects were still dark adapted, the effects of administering glucose were analyzed. Glucose was also given following the injection of insulin while the subjects were still dark adapted.

The results indicate that dark adaptation can be affected either by reducing the blood sugar with insulin or by lowering the partial pressure of oxygen, the effects being more striking in the latter. Following the injection of insulin or the deprivation of oxygen, the administration of glucose tends to increase light sensitivity in the dark adapted eye, although the effects are not completely counteracted by such procedures. At approximately 18,000 feet, the threshold for light sensitivity is raised by approximately 1 log unit, the extent of the change varying directly with the physiological characteristics of each subject.

The results suggest that dark adaptation may be impaired by reducing the amount of oxygen in the nervous tissue, either directly, by deprivation of oxygen, or indirectly, by lowering the blood sugar. The results also indicate that the changes are not concerned with the regeneration of the photo-sensitive substances of the visual mechanism, but with the neural elements of the retina and the central nervous system. [15 min., slides.]

2:45 P.M. *Stereoscopic Vision in Preschool Children.* L. F. BECK, University of Oregon.

In this experiment a more or less naturalistic setting was arranged where the child could reveal through his *spontaneous* reactions the presence or absence of stereoscopic vision. Twenty youngsters, two to six years old, viewed through polarized spectacles two superimposed, polarized, colored pictures of a doll projected life-size on a ground-glass screen. To an adult, the pictures produced a stereoscopic illusion of the doll's being held midway between the observer and the screen.

All the children, irrespective of age, reached for the doll several inches in front of the screen. That the stereoscopic illusion took on the appearance of reality was shown by verbal comments of which the following are typical: "I'm touching her nose but I can't feel her. Why can't I?" "I put my hand right through the dolly."

With the elimination of one image, the children then localized the doll at the plane of the screen. Some of the older ones spontaneously commented on the movement of the doll back to the screen, and the increase in the apparent size of the doll when seen at the greater phenomenal distance. These results show (1) that children as young as two years have well developed stereoscopic vision, and (2) the importance of convergence as a factor in the perception of size. Beth Johnson collaborated in this study. [15 min., slides.]

3:05 P.M. *Pitch and Frequency Modulation.* DON LEWIS, State University of Iowa.

This paper deals with an investigation of the pitch of sounds characterized by frequency modulation. It has to do mainly with the perceived extent of what may be called pitch glides. By means of a specially designed variable condenser, the frequency of an oscillator could be modulated (varied) in several different ways, with the intensity of output remaining essentially constant. Modulation of either linear or sinusoidal form could be obtained, and the amount and rate of modulation were both subject to control. It was possible to produce, among others, the following types of variation: (a) isolated frequency sweeps, either upward or downward in direction; (b) sweeps, either upward or downward, at the beginning or end of a steady-state sound; (c) periodic modulation, that is, frequency vibrato. Sounds with vibrato were studied only casually. Main emphasis was placed upon securing information on thresholds for bare perception of pitch change and on perceived extent of slides, always as a function of amount and rate of modulation.

The experimental data are interpreted to indicate their probable bearing on problems of pitch perception in speech and music. The data are further shown to have a significant bearing on auditory theory in that they reveal the great rapidity with which the ear responds to sounds of continuously and extremely rapidly varying frequency. An explanation of the results in terms of typical modulation theory is rather unconvincing. Doubt thus seems to be thrown upon the view that the ear can be regarded as a simple analyzer for short pulses of sound. An alternate view, based upon hydraulic principles and utilizing the concept of time rate of change of excess pressure, is set forth for consideration. (Assistance in the investigation was given by Grant Fairbanks, Milton Cowan, and Abe Pepinsky.) [15 min., slides.]

3:25 P.M. *The Attribute of Damping in Auditory Discrimination.* FRANKLIN HENRY, University of California.

The identification of objects by virtue of the characteristic sounds they emit when physically disturbed is probably based primarily on pitch and damping. The problem of discriminatory limits for environmental sounds of this type was approached by electrically producing damped tones in the earphones of human subjects and determining difference limens by a modification of the constant method. The present report is concerned with the difference limen for damping.

The average limen of 7 subjects for a 500 cycle sound having a damping coefficient of 30, ranged from 3.5 at 80 db. above threshold to 15.0 at 20 db. At 60 db. the Weber ratio was relatively constant as the coefficient was altered over the range 30 to 250. The ratio increased with increased damping at low intensities, the change being greatest between coefficients of 100 and 175.

The damping limen expressed as a temporal increment was substantially unaltered as the above-threshold duration of the tone was changed from .08 to .31 seconds by changing its intensity, but changed consistently when duration was altered by changing the damping level. This indicated that damping or some similar attribute (not duration as such) formed the basis of the discrimination. The difference limen for intensity was found to be of the order of 4 to 6 db. which was of sufficient magnitude to eliminate intensity difference as an important factor in judging difference in damping. [15 min.]

3:45 P.M. *The Relationship Between Age and Tonal Patterns in Song-Isolated Roller Canaries.* CONSTANCE CHANDLER, University of Southern California.

The purpose of this study was to analyze the song development of eight roller canaries raised under conditions of song-isolation, in order to determine the relationship between chronological age and (a) rate; (b) repetition; and (c) regularity of tonal patterns. Kymograms were made from phonograph records which contained representative song behavior for the entire period of song development. Tabulation of the number and rate per second of tonal patterns was made from the kymograms. The major findings were:

(1) Tonal patterns were sung, in general, at the same median rate and with the same variability of rate throughout the developmental period. (2) There was a relationship between the number of patterns of equal rate occurring in sequence and (a) chronological age; and (b) the age

of earliest song behavior. (3) There was a relationship between age and regularity of tonal patterns. [10 min.]

4:00 P.M. *The Measurement of Bird Responses to Degrees of Intensity of Auditory Stimulation.* MILTON METFESSEL, The University of Southern California.

The responses of a group of male roller canaries to varying intensities of call notes of a hen were individually recorded and measured during the mating season. A phonograph record with the call notes was played with an electrical device into a sound attenuation cage containing a male bird. Six wide steps of intensity were possible by means of a calibrated resistance unit in the input of the amplifier. These steps were numbered from 1 to 6 in order from greatest to least energy.

The original tests were followed by a control series, in which all conditions of the test series were maintained except that no sounds were played into the cage for steps 3, 4, 5, and 6. This gave a measure of the free song that would be expected during periods of no stimulation.

By this procedure, step 4 was determined as near the lower limit of bird song response to sound energy. The lower limit of hearing of a group of human observers occurred approximately in the region of step 3, with no hearing at step 4. No attempts have been made to measure minute differences between human beings and birds, as the interest in this study was restricted to determine whether bird responses to auditory energy would occur outside the same order of magnitude of sound energy as that at the lower limit of hearing of human beings.

This method offers comparable results to those obtained by a conditioning method, in which birds were trained to fly from a perch when a sound was generated, and which resulted in neurotic behaviors at low sound energies, worked out in collaboration with Mr. Lee Stockford. [15 min., slides, phonograph record.]

PHYSIOLOGICAL ASPECTS OF EMOTION AND MOTIVATION

Wednesday, September 6, 1:30 P.M.
Room 2, Education Building
GEORGE M. STRATTON, Chairman

1:30 P.M. *Pulse Rate Response of Adolescents to Ideational and Sensory Stimuli.* NATHAN W. SHOCK, University of California.

Changes in pulse rate after stimulation have been recorded in a group of 50 girls and 50 boys—aged 12 years. Two lists of 20 words each were administered to each child with instructions for free association. The experimental words were selected to contain four buffer, four pleasant, four unpleasant and four indifferent words determined by ratings of over one hundred adolescents. Ratings of pleasantness or unpleasantness of each word were obtained from the subjects during the test. Eight sound stimuli were presented to the subjects. Ratings of pleasantness-unpleasantness, and degree of startle produced were obtained. The sound stimuli included: harmonic chord, siren, automobile horn, gong and cap pistol shot.

Pulse rate was recorded photographically from a pneumatic cuff attached to the ankle of the subject. Preliminary analysis showed that if the pulse rate is counted over 10 or 15 second intervals, changes are missed; if one-second intervals are used chance fluctuations and errors in reading the records are large. An optimal interval of 3 seconds was used in the study. Reliability coefficients of pulse rate were found to be 0.85–0.95. All association word stimuli were followed by cardiac acceleration in the group average, with a maximum increase of 5 beats per minute which was attained 12 seconds after the stimulus was given. In contrast to changes of skin resistance, no significant differences between cardiac acceleration following pleasant, unpleasant or indifferent words were found. Sound stimuli produced a decrease in the average pulse rate of 3 beats per minute 3 or 6 seconds after the stimulus with a slight rise in rate at 12 seconds. The displacement was not significantly related to degree of startle reported by the subject. The physiological implications of this finding are discussed. [15 min., slides.]

1:50 P.M. *A New Approach to the Old Problem of Emotional Stability.* V. E. FISHER, Idaho State Hospital South, and R. I. WATSON, University of Idaho, Southern Branch.

This paper is based on a somewhat new conception of emotional stability, and presents the approach to the measurement of stability which the conception suggests. Emotional stability appears as the ratio existing between emotional arousability and emotional tolerance. By arousability we mean the individual's propensity to respond emotionally in his everyday life. Here we have at least three dimensions or variants: strength of response, duration of response and frequency of response.

An individual's emotional tolerance is his capacity to handle his emotional tension. Chief among the aspects of tolerance, found upon analysis, are the capacities to withstand intense or prolonged emotional tension, to vent or discharge emotional tension, and to govern or direct emotional tension.

Emotional stability, then, depends upon a balanced relationship between arousability and tolerance. A person with high tolerance can adequately cope with relatively high arousability. A person with low tolerance, on the other hand, possesses stability only if his arousability also is low. The person whose tolerance is relatively low and whose arousability is high is inevitably unstable.

An emotional stability inventory containing two kinds of items was constructed and given to more than three hundred college and school of education students. One hundred and twenty arousability items were scored on six-point scales for strength, duration and frequency. One hundred and fifty tolerance items were scored on a six-point scale for ability to withstand or to vent or to direct emotional tension.

From these total items those shown to be most valid by various statistical techniques were then given to fifty mild psychotic state hospital patients and to fifty psychoneurotic patients to derive an emotional stability quotient, expressed as, tolerance divided by arousability equals emotional stability quotient, or, simply, $\frac{T}{A}=ESQ$. [15 min.]

2:10 P.M. *A Comparison of the Weights of the Adrenal, Thyroid and Pituitary Glands of Emotional and Non-emotional Strains of Rats.* Calvin S. Hall, Western Reserve University.

The results reported in this paper were obtained in collaboration with Dr. Eleanor Yeakel aand Miss Ruth Rhoades of the Biology Department of Western Reserve University.

Two strains of rats, one emotional in behavior, the other relatively phlegmatic, are being selectively bred. The test of emotionality is defecation and urination in an open field situation. Five generations have been tested to date and the difference in emotionality between the strains in the fifth generation is marked.

A program of research has been initiated to determine the biological basis for this difference in emotionality. The first step in this program was a comparative study of gland size. The adrenals and thyroids of 55 F_3 and F_4 emotional rats and 55 F_3 and F_4 non-emotional rats were excised and weighed. The weight of the pituitary was determined for 30 F_4 emotional rats and 35 F_4 non-emotional rats. The average weights of these glands, relative to body weight, are greater for the emotional animals than for the phlegmatic rats. These results suggest that emotional behavior is associated with the endocrine system and that individual differences in gland size are inherited. [15 min., slides.]

2:30 P.M. *The Interrelationship of Six Different Situations in the Measurement of Emotionality in the Adult Albino Rat.* Milton M. Parker, Ohio State University.

The situations were: a brightly illuminated open field; a soundproofed box provided with a loud buzzer; an apparatus capable of administering a drop at approximately "g"; an apparatus equipped with a glass floor which, when tilted, threw the animal off balance and forced it to slide; a swimming tank; an apparatus which restricted the activity of the animal uniformly, and almost completely.

From the standpoint of the defecation response, all the situations were emotion-provoking. Standardization, on the basis of defecation in odd *versus* even trials, showed the reliability for each situation to be not less than 0.85.

Individual differences in the ability to adjust (inhibit defecation) were marked. Sex differences were negligible except in the case of the arena. For purposes of correlating the ability to adjust in one situation with that shown in adjusting to the other five, three successive trials with no defecation were set as the criterion.

The fifteen intercorrelations obtained between the various combinations of the six situations were all beyond Fischer's "one per cent point" of significance. Males and females correlated approximately alike. Correlation of the results of one situation with the pooled results of the remaining five showed no significant superiority for any single situation.

The consistency and statistical significance of the results argue strongly for the importance of the somatic determiners of emotionality. Despite this, however, the coefficients of alienation indicate a variance whose magnitude is probably larger than the experimental error involved in the six tests. [15 min., slides.]

2:50 P.M. *A Summary of Four Years' Work on the Social Behavior of Albino Rats.* ROBERT HALL BRUCE, University of Wyoming.

Problem: What is the effect of the presence of one or two rats upon the behavior of a single animal in the experimental field? Does shifting the amount of the thirst drive have any effect?

Procedure: Rats were run in an open field, and the following measures were taken: (1) time to water; (2) time of first drink; (3) excess distance covered; (4) amount of water consumed. All animals were motivated by varying amounts of water deprivation. Fisher "t's" were calculated to indicate reliability of means.

Results: The presence of a trained animal did not consistently aid the naïve animal to eliminate excess distance or decrease the time taken to reach the water. There was no evidence of tuitional learning nor can the concept "imitation" be legitimately applied. The time of the first drink was consistently and significantly less when two animals were in the experimental situation whether one or both could or could not drink from the same source of water. When the experimental conditions were such that both animals could drink at the same time from one source, the trained animal did not drink consistently more; when only one animal could drink from the source, the trained animals drank consistently more.

Giving ¼, ½, ¾ of the normal water intake per rat alternately to two groups did not significantly affect time to water, excess distance, or time of first drink. Satiation did affect these measures. The amount of water consumed varied with the amount given before the run. [15 min., slides.]

3:10 P.M. *The Innate Differential in the Social Learning of Fowls.* T. H. HOWELLS, University of Colorado.

Most studies of either innate or environmental factors involve a specified treatment or training for certain individuals while other equivalent individuals are not so treated. Differences which appear are commonly ascribed to training, while similarities which develop in spite of such treatment are commonly termed hereditary. Learning experiments involving equated experimental and control groups, and also studies of separated identical twins, are examples. In each instance the innate factor is supposedly held constant while the environmental factor is varied. The complement of this procedure in which the environment is kept constant from birth while the innate factors are varied has seldom been attempted. It was the purpose of this experiment to compare the social adjustments of two varieties of fowls toward own and other kind when the environments of both were equated from birth.

Equal numbers of two varieties of fowls (*e.g.*, chickens and ducks, or bantams and leghorns) were hatched at the same time from incubators, and were reared together in the same cage. When old enough individuals were trained in a Y discrimination box in which pairs of animals of own and other kind, from the rearing cage, served as cues indicating respectively food and shock, and *vice versa*. Animals completing this training were next trained (positively and negatively) to substitute cue animals of the same kind. The aim was to discover any tendency to generalize or react to kind, instead of to specific individuals, as shown by savings in relearning.

Results indicate tentatively that, with equal social contacts from birth, animals (1) learn to react differently to individuals of their own kind as compared with another kind, and (2) tend to generalize in terms of kind. [15 min., slides.]

3:30 P.M. *The Rôle of Hunger in the Filial Behavior of the Young Guinea Pig.* GEORGENE H. SEWARD, Connecticut College.

In the course of experiments on the drive of mother guinea pigs toward their young, we were impressed with the fact that the young seemed to show an even stronger drive toward their mothers. This raised the question as to whether this behavior is exclusively hunger-motivated. The present experiment was designed to throw light upon this question.

"Filial drive" was measured by the Hurdle Box technique previously described. According to this method, after an interval of separation from the mother, the young animal's drive is measured by the average time of crossing a barrier to reach her. Following this procedure we obtained records of normal filial behavior throughout the suckling period on 22 guinea pigs. The trend of these results was toward increased rather than decreased speed of crossing. This finding indicated stronger drive toward the mother in spite of the fact that the animals were presumably becoming less dependent upon her for nourishment.

In order to rule out hunger motivation more systematically a further refinement was introduced with a new group of six animals. In half the trials they were artificially fed milk to satiation during the separation interval. In the other half, they were merely given a corresponding amount of handling as a control. Although these results reveal greater drive on the "unfed" trials, there is, nevertheless, a strong residual drive even after feeding. Obviously this cannot be attributed to hunger. Interpretation in terms of social conditioning is suggested as a hypothesis for further experimentation.

This study was aided by funds from the National Research Council Committee on Sex Problems through the Department of Anatomy, Cornell University Medical College. [15 min., slides.]

TECHNIQUES OF MEASUREMENT OF TRAITS AND ATTITUDES

Wednesday, September 6, 1:30 P.M.
Room 521, Hydraulic Laboratory
Floyd H. Allport, Chairman

1:30 P.M. *Comparative Study of the Character Traits of Young Orphanage and Non-orphanage Children.* Leigh Peck, University of Texas.

Sixteen performance tests designed to measure phases of character development were adapted from the Hartshorne and May techniques for use with children at the nursery school, kindergarten, and primary levels. Honesty, persistence, inhibition, altruism, and social behavior were the phases of character studied. After the standardization of the techniques, groups of 71 orphanage and 84 non-orphanage children from three to ten years of age were tested. A retest of both groups was made at the end of seven months. The orphanage children showed a higher average of total character traits, but the non-orphanage children showed a greater improvement over the seven-month period. Both age and intelligence were positively related to scores on the character tests. [10 min.]

1:45 P.M. *A Study of the Interest-Attitude Test Scores of Delinquent Boys.* M. E. Odoroff, Division of Vocational Rehabilitation, St. Paul, Minnesota, and D. B. Harris, Stanford University.

Pressey Interest-Attitude test scores for 325 delinquent boys from the Minnesota State Training School average somewhat below the norms for corresponding age groups of unselected boys. This difference is somewhat less than that found by M. A. Durea, who studied a similar group of delinquents (*Jr. Abn. and Soc. Psychol.*, 1937). The average "emotional ages" of moron, borderline, and dull groups (classified on the basis of Kuhlmann-Binet I.Q.'s) are below the corresponding average chronological ages. This "emotional retardation" is, however, not so great as the mental retardation evinced by these same special groups. Correlational analysis reveals low positive relationships between emotional age and mental and chronological ages, respectively. Even though the Interest-Attitude test be accepted as valid, the reliability of younger subjects' responses perhaps may be questioned because of interpretative difficulties with a number of the stimulus words. [10 min.]

2:00 P.M. *Comparisons Between Self-Estimates and Classmates' Estimates of Personality During Adolescence.* CAROLINE MCCANN TRYON, University of California.

On a verbal portrait-matching (Guess Who) test each subject was instructed to match members of his class, including himself, to the descriptive pictures. There were some twenty pairs of word pictures, one in each pair describing the negative, inactive or undesirable aspect of a personality trait and the other the positive, active or desirable aspect. No one was forced to make judgments since the directions stated that the subject could skip any word picture if he could think of no one in the group who matched it. The test was administered eight times to the same group of children during a period of seven years (6th to 12th grades).

The data are analyzed in terms of (1) the incidence of self-mention for the group at each testing; (2) the degree to which agreement and disagreement between the individual's opinion of himself and his classmates' opinion of him is idiosyncratic; and (3) the tendency to restrict self-mention to acceptable or desirable qualities.

The analyses suggest the following general conclusions: (1) There is a marked tendency for self-mention to decrease as age increases; (2) for some of the traits it is as idiosyncratic to agree with one's classmates' opinion about oneself as it is to disagree; (3) there is a tendency to look more favorably upon one's own personality qualities than one's peers do, but this tendency varies widely among the traits. Factors related to this variation will be considered. [15 min., slides.]

2:20 P.M. *The Effect of a Knowledge of the Situation upon Judgment of Emotion from Facial Expressions Aroused in Everyday Life.* NORMAN L. MUNN, Vanderbilt University.

Candid camera photographs selected from back issues of *Life* and *Look* were made into lantern slides. The slides of one set pictured the whole situation. Those of the other contained only an enlargement of the subject's head. The latter slides were shown to 90 psychology students instructed to judge the subjects' emotional reactions. A week later these students judged the emotion expressed by the same faces, but while viewing them in their original setting. No terminology was suggested in either of these experiments. From the most frequent terms used by this group, a vocabulary for a further experiment was arranged. Sixty-five psychology students on another campus were given the same test, but this time with a suggested vocabulary to which their own terms might be added.

The predominant judgments of both groups were in most instances quite similar. Seven of the expressions were judged with an accuracy (agreement) of from 65 to 99 per cent by both groups and there was no significant change upon preceiving the situation. Two of these expressions were designated *joy*. The others were *terror, pain, anxiety, sur-*

prise, and *disappointment.* In the case of several other expressions, judgment changed markedly with knowledge of the precipitating circumstances. For instance, a predominant report of *sorrow* changed to *determination,* one of *anxiety* to *horror,* and one of *terror* to *surprise.* Some expressions aroused widely varying interpretations under both conditions.

Even where there was lack of agreement concerning the precise emotion being experienced, observers evidenced a marked ability to judge the affective tone of the expression. These experiments indicate the value of candid-camera material for research on emotional behavior. [15 min., slides.]

2:40 P.M. *Highlights in the Literature of Psychological Sex Differences Published Since 1920.* WINIFRED B. JOHNSON, GEORGE KUZNETS, and LEWIS M. TERMAN, Stanford University.

The report is based on a careful study of practically all the experimental literature on sex differences published in the English language since 1920. The areas covered include measured abilities, achievement of various kinds, and numerous personality variables. The resultant picture of the mental and behavior patterns exhibited by men and women of the white race in present-day Occidental culture constitutes a norm against which later changes in pattern may be evaluated. A striking improvement has taken place since 1920 in the quality of the investigations reported in this field. The authors are indebted to Olga McNemar for assistance in the conduct of the survey. [15 min.]

3:00 P.M. *The Comparative Efficiency of Various Methods of Weighting Interest Test Items.* ROBERT T. ROCK, JR., and ALEXANDER WESMAN, Fordham University.

Problem: To determine the relative efficiency of the weighting procedures commonly used in developing scoring keys for interest tests and to compare their efficiency with that of other possible weighting procedures.

Procedure: An interest-attitude test consisting of 100 items selected from the Terman-Miles Masculinity-Femininity Test was administered to 300 men and 300 women. Alternate cases were selected to form primary and secondary groups, each consisting of 150 men and 150 women. The responses of the primary groups were tabulated and weights for each of the possible responses to the 100 items were calculated by twenty different methods, including the following: the Cowdery-Kelley-Strong formula; the Kelley 1934 formula; critical ratio; critical ratio squared; the first four powers of per cent differences in response frequency; chi square; a modification of Flanagan's method of successive approximations; and a variety of "reduced" and "unit" weighting schemes. The weights thus calculated from the tabulated responses of the primary groups were then used for scoring the tests marked by the secondary groups. A measure of the efficiency of each weighting method in separating the secondary groups was obtained by determining from the total scores yielded by the scoring key based on each weighting procedure, the per cent of overlapping of the scores of men and women.

Results: The data of this study indicate that, in general, "reduced" and "unit" weighting methods are considerably less efficient in separating secondary groups than methods using larger and more variable scoring weights. The Cowdery-Kelley-Strong and the Kelley 1934 formulas did not differ in efficiency, and several weighting procedures were found to equal or surpass the effectiveness of these formulas. [15 min.]

GROWTH AND DEVELOPMENT

Thursday, September 7, 10:30 A.M.

Room 2000, Life Sciences Building, University of California

MAUD A. MERRILL, Chairman

10:30 A.M. *"Play Techniques" in Preschool as an Aid in Guidance.* DOROTHY WALTER BARUCH, Broadoaks School of Education, Whittier College.

Clinical procedures with "problem" children suggested the question: Can "play techniques" with "normal" children yield indications of emotional needs, and be of value in guidance?

Forty children were given play, under controlled conditions, with dolls representing their own family members. To make procedures compatible with usual preschool staffing, and hence usable in other schools, the investigation concerned itself with what might happen with each child in a fifteen minute period. Full records were taken both of child behavior and worker participation.

In many instances, revealing indications appeared. Often these had not shown in other phases of school behavior. They sometimes gave leads borne out by further investigation of family data. They sometimes corroborated staff hypotheses (based on parents' reports and school records) that children were reacting to certain tensions and pressures. The majority of cases yielded data of value in guidance. [10 min.]

10:45 A.M. *The Substitute Value of Activities for Children of Different Age Levels.* DAN L. ADLER, Iowa Child Welfare Research Station, State University of Iowa.

Observation indicated that young elementary school children react behaviorally to *identical* task-activities as if they were different. Older children seemed to react to identical task-activities at one time as if they *were* identical, at other times as if they were different. These reactions seemed to occur despite the children's ability to *perceive* the activities as the same.

To test the validity of these observations, sixty children between the ages of seven and eleven years were placed in each of three experimental situations as follows:

Control situation (I): Presentation of task A, interruption, presentation of task B physically and cognitively different from A.

Concrete situation (II): Presentation of task L, interruption, presentation of task M physically identical to L, but cognitively differentiated. L was referred to here as "Mr. Brown's house," M, as "Mr. Jones' house."

Categorical situation (III): Presentation of task Y, interruption, presentation of task Z physically identical to Y, and cognitively included in the same category. Thus Y and Z were referred to together as "house building material."

One would expect that the original task would be resumed when the task following interruption did not serve as a substitute for it. It was found that the majority of young children resumed the original task in all situations. The older children resumed this task in situations I and II, but not in the categorical situation. The biserial correlation between resumption in the categorical situation and chronological age was —.65; between resumption in this situation and mental age, —.38.

Despite the ability to *perceive* identical tasks as identical, the tendency to *react* to them as different is seen as a function of *chronological* age, and the type of situation presented. [15 min., slides.]

11:05 A.M. *Relationships Between Menarcheal Age, Behavior Disorders and Intelligence.* MARTIN L. REYMERT, Mooseheart Laboratory for Child Research, Mooseheart, Illinois.

The menstruation records of 138 girls who attained menarche at Mooseheart, and on whom we have exact information on every menstrual period thereafter, form the data of this report.

The median age at menarche is 13.1 years, Q=7.2 months. The correlation between the age at menarche and recent Binet I.Q. was —0.07±.08. For a selected group, 2½ years removed from menarche, there is little regularity approaching a once-in-28-days standard. Certain data concerning body type and menarche will be presented.

No significant difference was noted in the menarcheal age of problem *vs.* non-problem children, nor in the time elapsed since menarche. Nor were the problem children more irregular in the function.

The dependability of "recalled" data was checked. The correlation between actual age at menarche and the age recalled 1½ years afterwards was 0.79±.05. [10 min.]

11:20 A.M. *The Predictive Value of Several Different Measures of Mental Growth During the First Nine Years.* NANCY BAYLEY, University of California.

Forty-eight of the children in the First Berkeley Growth Study have been tested at 33 ages from birth through their ninth year. General tests of mental ability showed irregular progress in mental growth, especially during the first years.

In an attempt to find some measures of mental ability which would rate the children consistently during early childhood several combinations of scores were studied.

A "Developmental Score" made up of the sum of the items of the separate mental and motor tests yielded no greater consistency than did the mental test alone.

A selection of items in the California Preschool Mental Scale improved, slightly, the correlations of the two and three year tests with later performance, but the improvement was not great enough to produce constant scores over a period of four or five years.

Tests of vocabulary given from six to nine years of age gave scores moderately related to language tests at three and three and a half years, and were not significantly related to the age of first talking or to early mental test scores.

Tests of form-board and puzzle-board performance (at five and a half to eight and a half years), although related to mental test and vocabulary scores at similar ages, were unrelated to tests of ability during the first year.

It was concluded that mental organization changes with growth, and that this change is especially rapid before two years of age. [15 min., slides.]

11:40 A.M. *Sibling Resemblance in Intelligence, in Relation to Natal Interval.* HERBERT S. CONRAD, Institute of Child Welfare and College of Agriculture, University of California.

Problem: What is the comparative resemblance, in intelligence, of siblings close together in age *vs.* siblings farther apart in age? What are the implications of similarity *vs.* difference of resemblance in these two contrasted groups of siblings?

Procedure: A large unselected sample of children in rural New England were tested on either the Stanford-Binet or Army Alpha intelligence tests (occasionally both). Raw scores in each test were converted to sigma scores for each age-group. Correlations were computed between siblings of short natal interval (under three years); and between siblings of relatively long natal interval (three years or over).

Results: On the average, the correlations between siblings of long natal interval is about .03 less than the correlation between those of short (.52 *vs.* .49). An incidental finding is the higher correlation between opposite-sex siblings than between same-sex siblings. The results are based on a total of 778 sibling pairs (this becomes 1,556 by conventional double-entry for an intraclass r).

Interpretations: The findings are interpreted as indicating the absence of intra-sibship environmental influence on intelligence in the present sample. This does not preclude the possibility of inter-sibship influences, but weakens their probable significance. The possible effect of certain extraneous or spurious factors (*e.g.*, incomparability of Army Alpha *vs.* Stanford-Binet test, relation of parental age to offspring-intelligence, etc.) has been investigated, and found of either negligible or zero influence. A brief comparison is made between findings of the present study and results by other investigators.

Acknowledgment: The data for the present study were collected by Harold E. Jones, with the assistance of Mary C. Jones and the writer. [15 min.]

12:00 M. *Age, Race and Responsiveness of Levels of Aspiration to Success and Failure.* DONALD K. ADAMS, Duke University.

A group of 30 fourth-grade negro children was matched, child for child, with a group of 30 fourth-grade white children in respect of age, sex, educational status, mental age (hence I.Q.) and approximate economic and occupational status of parents. Similarly matched groups (also of 30 each) of whites and negroes were selected from the eighth grade and from college juniors. Each of these 180 individuals was the subject of a series of experiments including one on levels of aspiration. In this experiment the subject threw five darts at a target, added up his score, and was asked to predict his score for the next throwing of the five darts. This was continued until he had made 16 throws (of all five darts) and 15 predictions or "bids."

If "success" be arbitrarily (and not very psychologically) defined as a score equal to or greater than the subject's bid and "failure" as a score less than that bid, individual differences are found in a variety of relations. The one here reported is the percentage of cases of "success" followed by an increased bid, and that of "failure" followed by a reduced bid, which might be taken as a crude measure of "realism." The former percentage was about the same (about 70%) for both fourth grade groups and for the negro college juniors and higher (82%) for the white college juniors. The latter percentage (cases of failure divided into cases of failure followed by reduction of bid) was *lower* (43%) for both college groups than for both fourth-grade groups (white 63%, negro 57%). Possible interpretations of this and other findings are discussed. [15 min.]

12:20 P.M. *Successive Reproductions of Visually Perceived Forms.* ARTHUR BERMAN, Institute of Child Welfare, University of California.

This study investigates the reorganizations in the drawings of young children when visually perceived forms are reproduced to the point of satiation (rejection).

Twenty-five children—mean chronological age 46.9 months and mean mental age 56.34 months—acted as subjects. They made a total of 429 drawings.

The experimental procedure was composed of two aspects. Group I was shown the original form—a circle with three dots within the circumference representing a face—only during the first replication, and thereafter it was recalled verbally by the experimenter. Group II had access to the original for every reproduction for it was constantly in the field of vision. The subject continued his reproductions until he rejected the task.

The data reveal a mean of 14.66 reproductions in a mean time of 12.50 minutes for Group I. The mean number of reproductions for Group II was 19.46 reproductions in 12.68 minutes.

In summary: (1) Reorganizations in the successive reproductions occur in the drawings of all our children. Following Wulf, these changes may be classified as *sharpening, preserving*, and *leveling*. All of these processes function throughout a series of reproductions and all may appear in any single reproduction. (2) The tendency to *sharpen, preserve,* or *level* will continue when initiated until the satisfaction of the quasi-need, or the creation of a new reorganization. (3) A tendency in the drawings for increased complexity rather than simplicity was noted. (4) Reorganizations become more profuse in the distal stages of reproduction. Reproductions made in a satiated state are more disorganized than when no satiation is present. (5) The above factors operate similarly when the original is present in the visual field for every drawing.

It was concluded that satiation accentuates and distorts the normal process of graphic symbolization in the child.

Roma Tueller collaborated in this study. [15 min., slides.]

CONDITIONING

Thursday, September 7, 10:30 A.M.

Room 2003, Life Sciences Building, University of California

CLARK L. HULL, Chairman

10:30 A.M. *The Effect of Conditioning upon the Original Response to the Conditioned Stimulus.* LILLIAN DICK, Sarah Lawrence College.

The investigation was undertaken to discover whether the data reported by Erofeeva, to the effect that the original response to the conditioned stimulus disappears when conditioning takes place, would be substantiated if such factors as precision of measurement of the response, nature of the stimuli and adaptation were taken into consideration.

Four groups of college students were used as subjects. With the first three groups the conditioned stimulus was a tone of high intensity and the lid reflex was the measured response. The unconditioned stimuli for the three groups were, respectively: the same tone as the conditioned stimulus, a bright light and a shock. In the first two cases the measured response to the unconditioned stimulus and the conditioned response were the same as the response to the conditioned stimulus. In the third case, the unconditioned and the conditioned responses were psychogalvanic reflexes. With Group IV the shock was the conditioned stimulus and the tone the unconditioned stimulus. The effect upon the original response of varying the relative intensities of the conditioned and unconditioned stimuli was investigated. Accurate records were obtained by using the Dodge pendulum-photochronograph and a galvanometer.

The conditioned response was successfully established and extinguished in thirty subjects and appeared in the first two groups and the fourth as an anticipation of the unconditioned stimulus. The results clearly indicate the retention of the original response when conditioning has occurred and fail to show any consistent relationship between its latency, amplitude or development and the latencies, amplitudes and development of the conditioned and unconditioned responses. The theoretical significance of these findings will be discussed. [15 min., slides.]

10:50 A.M. *"Positive" and "Negative" Conditioning, Without Contraction of the Essential Muscles During the Period of Training.* W. N. KELLOGG, Indiana University.

The problem of the present investigation was to study the function of the effector organs in the learning of a reaction involving those effectors. Is movement necessary for learning, or can the organism develop a localized conditioned reaction without ever associating an actual muscular contraction (in the conditioned member) with the conditioned stimulus?

In the attempt to throw light upon this question, the buzz-shock method of conditioning was employed to train dogs to flex the right hind leg. Throughout all of the training some of the preparations were prevented from moving the member which was shocked by a temporary blocking of the motor pathways to the flexing muscles. This involved pinching off the motor roots of seven of the spinal nerves in such a way that rapid regeneration would follow after the training had been completed (Cajal, Light and Gantt). Other non-operated animals, subjected to a similar conditioning procedure, were prevented from flexing the limb which was shocked, by mechanical means. Retention tests were made after sufficient time to allow for complete recovery of the operated specimens.

The results showed that some of the subjects in both groups had learned to lift the previously unmoved member to the buzz stimulus even though they had never associated an actual flexion with the buzz. The behavior of these animals was characterized by great activity in the free or non-paralyzed muscles throughout the period of training. Some of the quieter subjects, on the other hand, developed conditioned inhibitions, and received the shock punishment without learning to make a conditioned withdrawal. Explanatory data on these two divergent sorts of integration will be presented. [15 min., slides.]

11:10 A.M. *An Experimental Investigation of Chained Responses in Indirect Conditioning.* ROGER BROWN LOUCKS, University of Washington.

The present experiment constitutes a test of Shipley's hypothesis that indirect conditioning can be accounted for in terms of responses which are chained to peripheral proprioceptive stimuli. Utilizing rabbits, a tone was made the signal for a conditional hind foot movement based upon shock to the toes. Secondly, the shock at the toes was employed as a signal for a conditional blink based upon shock at the cheek. Finally, the tone was administered alone to ascertain whether the conditional foot movement would be accompanied by a blink even though lid closure had never been associated directly with the tone. Capacitance "pick-ups" registered the movements on a long-trace cathode ray tube.

Indirect conditioning, in rabbits, follows upon a period during which the tone will evoke a conditional foot movement, and shock at the foot (evoking reflex foot movement) elicits a conditional blink—in the complete absence of chaining. Thus, a conditional blink is evoked only by the shock at the foot and not by a foot movement elicited as a CR to the tone. When the training routine is repeated several times, the tone, given alone, evokes a blink along with the conditional foot movement, even though the blink has never been directly associated with the foot movement. The lid response generally, but not invariably, trails the conditional foot movement.

As a final check upon the significance of peripheral proprioceptive cues, the animals were given a spinal anesthetic. Although all cues arising from the foot were thus eliminated, the tone continued to evoke the conditional blink. In rabbits, indirectly established CR's do not appear significantly dependent upon peripheral proprioceptive cases, and may even be demonstrated when the brain is largely isolated from the body. [15 min.]

11:30 A.M. *Thalamic Mediation in the Monkey of Affective Responses Capable of Being Conditioned.* JOHN A. BROMER, University of Wisconsin.

The localization of the cerebral receptive centers which mediate affective responses, and by inference affective consciousness, has never been conclusively demonstrated. The following experiment was designed as a critical test of the capacity of the thalamus to mediate affective responses after destruction of all appropriate cortical somesthetic centers.

Anatomical evidence indicates that the foot, arm, and face are separately represented in different thalamic areas and are discretely represented upon the cortex in the monkey. Thus cortical foot and arm somesthetic areas may be completely excised without destroying the cortical somesthetic representation of the face.

Frontal and parietal foot and arm areas were bilaterally extirpated in three monkeys. The block of extirpated tissue ran from the genu of the arcuate sulcus to the simian sulcus, and thus included any somesthetic foot or hand representation that may lie anterior to the central sulcus.

After operation, voluntary motion was limited to the face and neck. Vigorous movements of these parts resulted from painful or other forms of protopathic stimulation of the foot. Since all foot cortical representation had been destroyed, it was assumed that the facial responses to irritation of the foot were activated by thalamic sensory-affective centers.

Furthermore, head movements elicited by shock to the foot were conditioned to a bell stimulus in a few trials.

The existence of sensory-affective centers in the primate at subcortical levels furnishes additional evidence on the nature of emotional and motivational processes and their relation to learning.

These experiments were done in collaboration with Dr. H. F. Harlow. [15 min., slides.]

11:50 A.M. *Formation of Conditional Responses in the Cat and the Monkey During Inactivation of the Motor and of the Motor-somesthetic Cortex.* H. F. HARLOW, University of Wisconsin.

The purpose of this experiment was to determine the effect of complete inactivation of the motor and somesthetic cortex upon the formation of a conditioned emotional response.

The appropriate cortical areas were inactivated by freezing with ethyl chloride. The appearance of flaccid paralysis, abolition of reflexes (including the corneal), and complete loss of excitability to electrical stimulation were taken as indicators of temporary loss of function.

Training for conditioning was carried out in four monkeys during inactivation of motor and premotor areas and in two monkeys and two cats while somesthetic and motor areas were frozen. The CS, a bell, was sounded for two minutes and overlapped the half-second presentation of the US, a faradic shock to the leg. From six to ten training trials were given in a single training period, and as many as three training periods, always with the appropriate cortical areas frozen, were utilized.

In spite of the fact that ethyl chloride freezing always resulted in some residual paralysis, conditioned responses were obtained in all subjects. These conditioned responses ranged in intensity from forceful closure and flutter of the eyelids to struggling movements involving the entire body.

These data strongly favor an "intra-cerebral" theory of learning, since no movement ever appeared to the conditioned stimulus and since inactivation of somesthetic areas eliminated the possibility of spatially localizable proprioceptive stimuli fulfilling any function in the conditioning process.

Inferentially, these data bear on the motor theories of consciousness, and on the various theories of emotion which assume that proprioceptive sensations mediated by cortical centers play some essential rôle in awareness of learned or unlearned emotions.

These experiments were done in collaboration with Mr. John A. Bromer. [15 min., slides.]

12:10 P.M. *Pupillary Conditioning to Auditory Stimuli Near the Absolute Threshold.* **Ernest R. Hilgard** and **John A. Ohlson**, Stanford University.

The experiments were designed to test Baker's finding that pupillary responses may be conditioned in a few trials to auditory stimuli near the absolute threshold. Intensities of stimuli and time intervals comparable to his were used, although there were some modifications in procedure. Attempted conditioned constriction (10 subjects) and conditioned dilation (10 subjects) failed when the fixation object was 1 meter from the eyes. When, however, the fixation object was placed at 15 cm. from the eyes, positive results were obtained within 8 reinforcements for both conditioned constriction and dilation in a majority of the 20 subjects tested under these circumstances. The data suggest that pupillary changes associated with convergence upon a near fixation point may be important in determining the conditioned responses. [10 min., slides.]

12:25 P.M. *The Effect of Inhibitory and Facilitatory Attitudes on Eyelid Conditioning.* **Joseph M. Miller**, Institute of Human Relations, Yale University.

The hypothesis that voluntary attitudes in human subjects determine conditioning scores was tested with three groups of subjects. A conditioned lid response to light, based on the reflex wink to air-puff, was studied. One group (I) was instructed to inhibit lid movements, another group (F) was instructed to refrain from any voluntary control of lid movements, and the third group (C) was given no instructions regarding voluntary behavior. Fifty pairings of light and puff were followed by 20 extinction trials (light alone).

The inhibitory attitude resulted in reduced conditioning and rapid extinction, and the facilitatory attitude (group F) led to rapid conditioning and delayed extinction. Group F yielded the most and group I the least conditioning by all criteria. Differences in frequency of appearance, amplitude and latency of response were all reliable. Means of group C were close to those of group I, indicating a predominantly inhibitory attitude in the uninstructed subjects. Differences between means of groups F and C were reliable, those between C and I were not. Inhibitory attitude was correlated with objective response characteristics.

Continuous increment in conditioning during successive 10 pairings in group I gave evidence that the inhibitory set became less and less effective due to repeated reinforcement of the antagonistic CR tendency. Implications with regard to the voluntary control of CR's are stated.

Extinction was most rapid in group I and slowest in group F. A reliable decrement resulted in all groups after 20 non-reinforcements. [15 min., slides.]

12:45 P.M. *Semantic, Syntactic, and Phonetographic Generalization of Verbal Conditioning.* G. H. S. RAZRAN, Columbia University.

Salivary CRs were established in 6 adults to words and sentences flashed on a screen at random order while the subjects were consuming food in 2-minute periods. 'The relation between memory and digestions' was the "given" purpose of the experiment so that the subjects attended carefully to their tasks while unaware of attempts to condition them. The conditioning developed quickly and on the second experimental session transfer tests to words and sentences that had not been specifically associated with the eating were begun. The transfer words were related to the conditioned words either only semantically or only phonetographically (sound-spelling) or through some combination of the two: *e.g., style, fashion, stile; dark, night mark; take-took, bake-book; lock-latch, mock-match.* The conditioned sentences consisted of 3 words—subject, copula, predicate—and in the transfer tests one or more of the 3 were reversed giving rise to 7 verbal variations and to 4 contradictory and 3 concordant statements, *e.g., Poverty is degrading:—Wealth is degrading, Poverty is not degrading, Poverty is not uplifting, Wealth is uplifting,* etc.

Results.—For single-word conditioning, semantic factors were the most potent determinants of the salivary CR transfer or generalization, although phonetographic relationships were also very significant, especially at the beginning of the conditioning and after an excess of it. For the sentences, the order of the dependence of the transfer was general agreement of statement, agreement of copula, predicate, subject, but reversals of copulas interfered more with transfer than general reversals of statements. Conditioned single words lost some of their CR strengths when combined in sentences and subjects' opinions about truths of statements had some effect upon conditionings and transfers of sentences. [15 min., slides.]

ROUND TABLES

MONDAY, SEPTEMBER 4, 4:00 P.M.

The Elementary Course in Psychology. HERBERT S. LANGFELD, *Chairman.* Room 268, Engineering Building. Participants: Warner Brown, Leonard Carmichael, Harold R. Crosland, Elmer K. Culler, John F. Dashiell, Knight Dunlap, R. M. Elliott, Paul R. Farnsworth, J. P. Guilford, E. R. Guthrie, E. R. Hilgard, Donald G. Marquis, John A. McGeoch, E. B. Newman, Willard L. Valentine, William R. Wilson.

TUESDAY, SEPTEMBER 5, 4:00 P.M.

The Meaning of Psychological Meaning in Mental Measurement. PHILIP J. RULON, *Chairman.* Room 521, Hydraulic Laboratory. Arranged by the Program Committee of the Psychometric Society. Participants to be announced.

The Psychology of International Relations and the Prevention of War. NORMAN C. MEIER, *Chairman.* Room 268. Engineering Building. Arranged by the Program Committee of the Society for the Psychological Study of Social Issues. Participants: Floyd H. Allport, Paul R. Farnsworth, Ralph H. Gundlach, Kurt Lewin, Ross Stagner and George M. Stratton.

Relation of Conditioning to Verbal and Voluntary Behavior. ELMER K. CULLER, *Chairman.* Room 50, English Building. Participants: G. H. S. Razran, *Discussion Leader;* Leonard Carmichael, Ernest T. Hilgard, Clark L. Hull, Donald G. Marquis, Carl N. Rexroad.

WEDNESDAY, SEPTEMBER 6, 4:00 P.M.

The Psychology of Industrial Conflict. EDWARD K. STRONG, *Chairman.* Room 2, Education Building. Arranged by the Program Committee of the Society for the Psychological Study of Social Issues. Participants: Paul S. Achilles, Franklin Fearing, Edwin R. Guthrie, George W. Hartman, Arthur W. Kornhauser, Irving Lorge, and Goodwin Watson.

BUSINESS MEETINGS AND PRESIDENTIAL ADDRESSES

ANNUAL BUSINESS MEETING
OF THE
SOCIETY FOR THE PSYCHOLOGICAL STUDY OF SOCIAL ISSUES

GEORGE W. HARTMANN, *Chairman*

SUNDAY, SEPTEMBER 3, 7:00 P.M.

AUDITORIUM, EDUCATION BUILDING

ADDRESS OF THE CHAIRMAN
SOCIETY FOR THE PSYCHOLOGICAL STUDY OF SOCIAL ISSUES

VALUE AS THE UNIFYING CONCEPT OF THE SOCIAL SCIENCES

GEORGE W. HARTMANN

COLUMBIA UNIVERSITY

SUNDAY, SEPTEMBER 3, 9:00 P.M.

AUDITORIUM, EDUCATION BUILDING

ANNUAL BUSINESS MEETING
OF THE
PSYCHOMETRIC SOCIETY

T. L. KELLEY, *President*

MONDAY, SEPTEMBER 4, 4:00 P.M.

AUDITORIUM, EDUCATION BUILDING

ANNUAL BUSINESS MEETING
OF THE
AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

GORDON W. ALLPORT, *President*

TUESDAY, SEPTEMBER 5, 8:00 P.M.

AUDITORIUM, EDUCATION BUILDING

ADDRESS OF THE PRESIDENT
AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

THE PSYCHOLOGIST'S FRAME OF REFERENCE

GORDON W. ALLPORT

HARVARD UNIVERSITY

THURSDAY, SEPTEMBER 7, 2:30 P.M.

ROOM 2000, LIFE SCIENCES BUILDING, UNIVERSITY OF CALIFORNIA
BERKELEY, CALIFORNIA

The Members of the Department of Psychology at Stanford University and the University of California will entertain the Members of the Association and their guests at an informal smoker in the Psychological Laboratory after the Presidential Address.

GENERAL ANNOUNCEMENTS, LUNCHEONS, AND DINNERS

Note: Additions to this list will be posted at Headquarters.

GENERAL

Questions concerning local arrangements should be referred to Dr. Calvin P. Stone for Stanford University and Dr. Warner Brown for the University of California.

Reservations for dormitory accommodations should be sent to Dr. E. R. Hilgard, Stanford University, California, well in advance of the meeting dates, giving specific information as to requirements.

Advance arrangements for luncheon groups can be made by writing Dr. Paul R. Farnsworth, Department of Psychology, Stanford University, California.

Apparatus and books will be exhibited at Stanford, in Room 300, Engineering Building. Persons wishing exhibit space should communicate with Dr. E. R. Hilgard, Department of Psychology, Stanford University, the local chairman of exhibits. Exhibitors will be responsible for packing and unpacking their materials and for the cost of shipment.

Questions concerning the program and business meeting of the American Psychological Association should be referred to Dr. Willard C. Olson, Secretary, University of Michigan, Ann Arbor, Michigan.

Questions concerning the program and business meeting of the Psychometric Society should be referred to Dr. Jack W. Dunlap, Secretary, University of Rochester, Rochester, New York.

Questions concerning the program and business meeting of the Society for the Psychological Study of Social Issues should be referred to Dr. I. Krechevsky, Secretary, University of Colorado, Boulder, Colorado.

The Department of Psychology at the University of California at Los Angeles invites psychologists in attendance at the Annual Meeting to visit the new psychology building which should be structurally complete by the time of the meetings. Members of the staff will be available for entertainment of visitors before and after the scheduled programs. Inquiries should be directed to Professor Knight Dunlap, Chairman, Department of Psychology, University of California at Los Angeles. (Santa Monica telephone directory.)

MONDAY, SEPTEMBER 4

There will be a luncheon for alumni of Stanford University at the Stanford Union, at 12:00 o'clock, Monday, September 4. Tickets at 65 cents should be secured at the registration table.

The Psychometric Society dinner meeting will be held Monday, September 4, 1939, at 6:00 P.M. in the Stanford Union. For reservations at $1.00 each, write to Dr. Jack W. Dunlap, Department of Education, University of Rochester, Rochester, New York.

TUESDAY, SEPTEMBER 5

Members of Psi Chi, national honorary society in psychology, will hold a luncheon meeting at the Stanford Union, Tuesday, September 5, at 12:30 P.M.

Psychological Corporation Luncheon Meeting will be held Tuesday, September 5, at 12:00 in the Stanford Union. For reservations, write to Dr. Paul S. Achilles, Psychological Corporation, 522 Fifth Avenue, New York City.

Members of the Department of Psychology of Stanford University and the University of California will hold an informal reception for members and guests of the American Psychological Association from 4:00 to 6:00 o'clock on Tuesday, September 5, in the Laurence Frost Amphitheater.

THURSDAY, SEPTEMBER 7

Arrangements with the Atlas Travel Service, Berkeley, have been made to provide busses as needed to transport delegates from Stanford to the University of California on the morning of Thursday, September 7, in time for the morning sessions. The Atlas Service will sell tickets at Stanford in advance so that all wishing this service will be provided with transportation.

A general noon luncheon has been arranged for members and guests of the American Psychological Association for Thursday, September 7, at 1:15 P.M., at the International House (end of Bancroft Way, Berkeley). Tickets at 65 cents should be secured in advance at Headquarters.

University of Chicago Luncheon will be held Thursday, September 7, at the International House at 1:30 P.M.

NOTES AND NEWS

THE University of Colorado Summer School announces a *Conference on the Psychology of Learning,* to be held in Boulder, Colorado, August 9–11, 1939, under the direction of Professor K. F. Muenzinger and Dr. I. Krechevsky of the University of Colorado psychology department.

The University extends a cordial invitation to attend this Conference to all psychologists who will be in the vicinity during that period. The program follows:

Wednesday, August 9, 8:00 P.M.
  E. C. TOLMAN, University of California: "Motivation in Learning."

Thursday, August 10, 3:00 P.M.
  E. C. TOLMAN, University of California: "Trial and Error Learning."

Thursday, August 10, 4:00 P.M.
  Panel Discussion of Trial and Error Learning:
    K. F. MUENZINGER, Chairman; R. W. BRUCE, University of Wyoming; S. RENSHAW, Ohio State University; R. W. HUSBAND, University of Wisconsin; C. C. DOVE, New Mexico State College.

Thursday, August 10, 8:00 P.M.
  N. R. F. MAIER, University of Michigan: "Habit Conflicts and Experimental Neurosis in Animals."

Friday, August 11, 3:00 P.M.
  N. R. F. MAIER, University of Michigan: "Problem-Solving in Animals and Men."

Friday, August 11, 4:00 P.M.
  Panel Discussion of Problem-Solving:
    I. KRECHEVSKY, Chairman; R. A. DAVIS, University of Colorado; Ross STAGNER, Dartmouth College; E. B. NEWMAN, Swarthmore College.

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Psychological Bulletin

PROCEEDINGS OF THE THIRTY-FOURTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

C. P. HEINLEIN, SECRETARY, FLORIDA STATE COLLEGE FOR WOMEN

The Thirty-fourth Annual Meeting of the Southern Society for Philosophy and Psychology was held in Durham and in Chapel Hill, North Carolina, on April 7 and 8, 1939. The host institutions were Duke University and the University of North Carolina. The headquarters for the Society was the Washington Duke Hotel in Durham.

The opening meeting of the Society on Friday morning, April 7, at 9:00 A.M., was grouped into 3 separate sections, 1 in philosophy and 2 in psychology, and was held in assembly halls of the Washington Duke Hotel. Five papers in philosophy and 14 papers in psychology were read during the morning sessions.

By invitation from Duke University, a luncheon was given to members and guests of the Society at the Women's Union, East Campus of Duke University.

On Friday afternoon, between 2:00 and 4:00 P.M., 3 separate sections, 1 in philosophy, represented by 5 papers, and 2 in psychology, represented by 10 papers, met in the School of Religion and in the Biology Building, located on West Campus of Duke University. Members and guests of the Society were extended an invitation from Duke University to attend Memorial Exercises, which were held on Friday afternoon at 4:00 P.M. in honor of the late Dr. William McDougall, formerly professor of psychology at Harvard University and Duke University.

On Friday evening at 7:00 P.M. the annual banquet of the Society took place in the Crystal Ball Room of the Washington Duke Hotel. Following the banquet, Dr. Frank A. Geldard delivered his presidential address, entitled "Explanatory Principles in Psychology." President Geldard was introduced by Dean Marten ten Hoor.

On Saturday morning at 9:00 A.M. a general session, represented by 5 papers in both fields of philosophy and psychology, was attended by the Society as a whole in the main assembly hall of the Washington Duke Hotel. The annual business meeting followed the general session, with President Frank A. Geldard presiding.

By invitation from the University of North Carolina, a luncheon on Saturday was given to members and guests of the Society at the Carolina Inn, Chapel Hill. The luncheon was followed by the Saturday afternoon session, divided into 3 sections: 1 in philosophy, represented by 5 papers, and 2 in psychology, in which 11 papers were read. Following the regular sections in psychology and philosophy, a special section in Extra-Sensory Perception was held in Peabody Building on the Chapel Hill campus.

Members who served as chairmen of the various sections which convened throughout the 2 days are as follows: General Session—President Frank A. Geldard; Philosophy—Alban G. Widgery, George Thomas, and Stephen A. Emery; Psychology—John Frederick Dashiell, Karl E. Zener, A. G. Bayroff, William A. Brownell, Helge Lundholm, and Robert J. Wherry. J. B. Rhine served as chairman of the special section in Extra-Sensory Perception.

The Council of the Society met in executive session on Thursday, April 6, at 8:00 P.M., in the Washington Duke Hotel. President Frank A. Geldard presided over the meeting. Other Council members attending the meeting were: Emily S. Dexter, Marjorie S. Harris, C. P. Heinlein, Marten ten Hoor, Fritz Marti, James B. Miner, and Herbert C. Sanborn.

MINUTES OF THE ANNUAL BUSINESS MEETING

The minutes of the Thirty-third Annual Business Meeting of the Society, as published in the *Psychological Bulletin,* Vol. 35, No. 8, October, 1938, were approved as printed.

The annual report of the Secretary was approved as read. The report contained a description of the various tasks which the Secretary fulfilled during the year 1938–1939. Among the tasks listed was that of extending invitations to prospective members. A total of 218 invitations to join the Society were extended to persons assumed to be qualified for membership in various States of the South.

An analysis of membership during the year 1938–1939 revealed a gain of 36 members by virtue of affiliation with either the American Psychological Association or the American Philosophical Association. The names and addresses of the 36 members are as follows: C. M. Allen, Oklahoma City University; A. G. Bayroff, University of North Carolina; E. D. Bridges, Woman's College, University of North Carolina; P. A. Brown, Alabama Polytechnic Institute; J. Calvin, Yale University; E. E. Cowles, University of Alabama; M. P. Crawford, Yale Laboratories of Primate Biology, Florida; H. H. Dubs, Duke University; J. H. Elder, Yale Laboratories of Primate Biology, Florida; S. A. Emery, University of North Carolina; T. J. Farr, Tennessee Polytechnic Institute; M. McK. Font, Tulane University; M. Freyd, Social Security Board, Washington, D. C.; W. E. Galt, The Lifwynn Foundation, New York City; E. Ghiselli, University of Maryland; E. H. Henderson, Meredith College; J. G. Jenkins, University of Maryland; F. Jennings, Ocala, Florida; F. G. McLarty, Duke University; C. B. McMullen, Centre College; D. Meiklejohn, College of William and Mary; E. C. Milner, Guilford College; E. F. Möller, Sweetbriar College; G. A. Morton, Jr., Duke University; C. Mosier, University of Florida; R. S. Musgrave, Russell Sage College; E. Newbury, University of Kentucky; W. A. Owings, Textile Junior College; M. G. Rigg, Oklahoma A. and M. College; R. W. Russell, University of Virginia; F. C. Shepard, Guilford College; R. T. Sollenberger, Yale University; A. Waite, Rollins College; L. R. Witmer, Florida State College for Women; M. O. Wilson, University of Oklahoma; and P. Worchel, Florida State Hospital.

There was a loss of 3 members during the year: 1 member resigned in good standing and 2 members were lost through death (William McDougall of Duke University and Michael Demiashkevich of George Peabody College).

The report of the Treasurer was approved as read. Assets and receipts totaled $1,261.99, expenses $344.11, leaving a cash balance at the end of the fiscal year, April 3, 1939, of $917.88. This represents a net gain during the year of $42.80, the cash balance at the end of the fiscal year, April 9, 1938, having been $875.08.

Professor A. G. A. Balz, Chairman of the Standing Committee on Philosophy, gave a report of the work which the Committee had undertaken during the year 1938–1939, together with a brief description of the problems which the Committee had outlined and proposed to study in the future. Professor Balz announced that one of the objectives of the Committee was the investigation of the status of philosophy in southern institutions. Letters were sent to the heads of 122 institutions in the South for the purpose of securing from these authorities expressions of attitude towards philosophy from several points of view, and of eliciting indications of plans and policies relative to the teaching of philosophy. The Committee planned to make a study of the replies, 60 of which had been received from institutional heads at the time the announcement was made, and to present an informative report of the study to the Society at its next meeting.

A supplementary announcement was made to the effect that a new journal, *Philosophic Abstracts,* was to be established to cover the field of philosophy in all languages in which philosophy is cultivated and that the first number would appear in the Fall of 1939.

No formal report was made by the Standing Committee on Psychology.

Under the heading of unfinished business, 2 amendments to the Constitution, as approved and tabled in accordance with Article V at the Thirty-third Annual Business Meeting, were presented to the Society for its consideration and final action. The following amendments, relating to reinstatement of members and election regulations, were passed upon by the Society and became law:

Amendment to Article II, Section 2, of the Constitution:

> "Any former member who seeks reinstatement must make application to the Council. If such a former member was dropped for nonpayment of dues he will be required upon re-election to pay such dues as he was in arrears at the time when he was dropped from the Society."

Amendment to Article III, Section 3, of the Constitution:

> "Vacancies in all offices shall be filled at the annual meeting of the Society upon nominations made by the Council and by the members of the Society at its annual meeting."

Under the heading of new business, the Council offered to the Society the recommendation to rescind recommendation No. 2 (p. 488 of the published proceedings of the Thirty-third Annual Meeting), which reads:

> "2. That the arrangement of the program for the annual meeting, as far as philosophy papers are concerned, be assigned to the Standing Committee on Philosophy."

The Society moved and passed to accept the Council's present recommendation to rescind the above quoted recommendation, made in 1938.

The Council recommended that Article II, Section 2, of the Constitution be amended to read as follows:

> "Candidates for membership shall be proposed by two members of the Society and recommended by the Council before their names are voted upon by the Society."

The Society approved acceptance of the recommendation. Approval of the amendment was automatically tabled for final action by the Society at its next annual business meeting.

The Society passed a motion to appropriate the sum of $50 to assist the Committee of the American Psychological Association concerned with the employment of displaced foreign psychologists.

On recommendation of the Council, the following new members were elected to the Society: L. W. Beck, Emory University; E. A. Becknell, Tampa, Florida; M. F. Burts, Spartanburg, South Carolina; P. W. Cobb, National Research Council; R. M. Flory, University of Virginia; A. D. Grinsted, Louisiana State University; H. G. McCurdy, Milligan College; D. H. Ramsdell, University of Alabama; A. C. Reid, Wake Forest College; A. D. Roberts, Greendale, Kentucky; C. B. Scarborough, Florida State College for Women; H. S. Smith, Duke University; M. G. Walker, University of Georgia; and J. Weitz, University of Virginia.

On recommendation of the Council, the following officers and members of the Council were elected by the Society:

President: Marjorie S. Harris, Randolph-Macon Woman's College; Secretary-Treasurer: Norman L. Munn, Vanderbilt University; Council Members: Lewis M. Hammond, University of Virginia (1939–1942); C. P. Heinlein, Florida State College for Women (1939–1942); L. O. Kattsoff, University of North Carolina (1939–1940).

A Council recommendation to accept the invitation of Tulane University as host institution for the 1940 meeting was approved by the Society.

Professor Buford Johnson, Chairman of the Committee on Resolutions, moved the following resolution:

> "Be it resolved: That the Southern Society for Philosophy and Psychology expresses its great appreciation to Duke University, the University of North Carolina, and the Local Committee for their cordial hospitality and the excellent arrangements made for this thirty-fourth meeting of the Society."

The motion was responded to with a rising vote of approval, and the meeting was adjourned.

PROGRAM

FRIDAY MORNING SESSION, APRIL 7

PHILOSOPHY

CHAIRMAN, ALBAN G. WIDGERY

The Concept of Wholeness in Ethics. LEWIS W. BECK, Emory University, Emory University, Ga.

Like the biologists and psychologists who have had to supplement their analytical investigations with reference to the properties of wholes, the moralist must consider wholes of value as conditions in the evaluation of single items of worth. The quantification of values with reference to certain abstractions, as though evaluations were independent of each other, is rejected in favor of the recognition of systems or wholes of value as the basis for choice. But the appeal to a whole as a condition of the properties of parts is significant only if there is an objective criterion of wholeness. This is found in a "Principle of Inhomogeneity," which is like, but broader than, Moore's "Principle of Organic Unities." As in Moore's concept of whole, there is an appeal to intuition, but this theory differs from Moore's in holding that the values of objects may depend in part upon their relations, and for this dependence certain criteria are suggested.

Religious Values. W. PRESTON WARREN, Furman University, Greenville, S. C.

The problem of religious values is among the most unsettled in the total field of values. Defining values as validities, however, it is possible to distinguish both religious validities and invalidities from a number of approaches. Following Macintosh and others, one may demark moral, aesthetic, social, political, hygienic, and intellectual, besides distinctively religious values in religion at its best. Uncertainty is greatest concerning the intrinsically religious values. Scheler, Otto, Leighton, Brightman, *et al.* set the problem in relief. But essential phases of religion (faith, vision, and devotion, in their cosmic reaches) show the genius of religious values. Religions and philosophies are functions of each other. Religious values are the values from man's consciousness of cosmic loyalties, functions, and relations (his trust in goodness and in God, and loyalty to what he takes to be the cosmically highest). God is highest of all values, but not the greatest value. The kingdom of God includes God and all other positive values. Religion not only has its own distinctive values, but it is singularly productive in *Strahlwerte*.

Individualism in the Philosophy of Nietzsche. GEORGE ALLEN MORGAN, JR., Duke University, Durham, N. C.

Against the crude view of Nietzsche as an apostle of violence, deaf to spiritual values, and against the subtler conception of him as an extreme individualist, I hold that individuality is one of his supreme values, yet neither universal nor absolute. The cardinal principle of his social philosophy is *Rangordnung*, the recognition of many grades of human nature, each with its own appropriate ethic. Of these, the highest consists of "individuals," not in the trivial logical sense, but in the distinctive sense: "integrity of independent, self-contained personality." Such beings are rare, and Nietzsche's individualism is for the select few only. Nor are they splendid savages: They arise preëminently as heirs of a ripened social discipline, of a corporate life whose accumulated riches they incarnate. But *when* they arise they have still to make, each for himself, the transition from sociality to perfected individuality. For this purpose the moral philosophies of the past have at best been delusive group-moralities in disguise. In their stead Nietzsche formulates an "individual morality" proper, whose chief imperative is: "Become what you are," and whose culmination is a life in which freedom and necessity are one, an Apollinian god appearing within a Dionysian World.

A Precise Meaning for Objective and Subjective in Value Theory. HAROLD N. LEE, Sophie Newcomb College, New Orleans, La.

If value and being valued are the same thing, value is subjective. This offers no definition of "subjective," however. Many persons suppose, then, that if there are unappreciated values, values must be objective; but this offers no definition of "objective." Neither can the terms be defined by speculating about what would exist if no consciousness existed.

"Objective" can best mean: granted the existence of a consciousness, the *sufficient* conditions of the experience of value are independent of the reactions of that consciousness. "Subjective" can mean: granted the existence of a stimulus, the *sufficient* conditions of the experience of value are to be found in the reactions of consciousness to that stimulus.

If one defines "objective" and "subjective" in these ways, it is apparent that inescapable considerations stand in the way of defining value in either term alone. Objective conditions are necessary, but not sufficient. Subjective conditions are necessary, but not sufficient. The experience of value requires a definite relation between the 2 sets of necessary conditions. Although the experience of value depends upon this relation, value is defined as a potentiality. Thus, value and being valued are not the same thing.

Factual and Valuational Judgments in Ethics. HELMUT KUHN, University of North Carolina, Chapel Hill, N. C.

According to a widely accepted view, the ethicist must first establish the facts of human conduct and then, in a second phase of his work, relate these facts to values. I propose to point out that a dualistic method, which strictly separates the realm of facts from the realm of values, is inadequate. As a criterion may serve the questions: What is an action? Wherein does its oneness consist? First, the action must be marked off against its consequences. To draw this demarcation line we have to recur to the idea of responsibility, thus availing ourselves of an element of valuation. In the second place, an action must be distinguished as one and as distinct from others within the series of actions making up the whole of a human life. This leads to the supposition of different classes of actions with respect to their greater or lesser comprehensiveness in such a way that each class corresponds to a specific layer of personal life. Such a discrimination of various strata of activity differing from each other in 'depth' is again not to be conceived without an admixture of valuation. The conclusion is: The horizon, within which we have to define the basic notions of ethics, is circumscribed by a total conception of human life—a conception which cannot be classed with factual statements.

PSYCHOLOGY

SECTION I

CHAIRMAN, JOHN FREDERICK DASHIELL

Laboratory Exercises in Animal Behavior. EDWARD NEWBURY, University of Kentucky, Lexington, Ky.

Difficulties standing in the way of student exercises for a laboratory course in animal psychology include the necessity for technical skill on the part of experimenters in controlling distraction, the large number of animals required, the successive long periods needed to complete single experiments, and the decline in motivation of an animal when given numerous rewarded trials in quick succession. With the white rat these difficulties can be largely overcome by proper selection and planning of experiments for class purposes and by requiring the animal to escape from water in the experimental situation. Based on wading or swimming, the method has been found applicable to a variety of student experiments exhibiting principles and techniques in learning, discrimination, orientation, work, and motivation. Initial swimming behavior and reflexes in infant and adult rats permit the study of developmental factors. Other experiments extend the course work to problems in which the water technique is not feasible. Each experiment can be performed in a single laboratory period. Procedure and apparatus can be simple, an omnibus tank serving several exercises.

Tests of Imitation in Chimpanzees. James H. Elder, Yale Laboratories of Primate Biology, Orange Park, Fla.

Formal tests of imitation in animals rarely provide clear evidence of the existence of such ability. Failure to secure positive results may be due to specific features of method which cause lack of motivation, lowered attention, or excessively abstract relations between the essential acts and the goal. This report describes some simple tests which have been used in the study of imitation in chimpanzees. Results indicate that dominance status may be important in determining the relative ease or difficulty of learning a problem. Several subjects achieved success after 1 demonstration, while others failed to learn after repeated demonstrations. It appears that learning occurs more quickly when the demonstrator is dominant over the imitator.

The Relative Effectiveness of 2 Conditioning Procedures. Norman L. Munn, Vanderbilt University, Nashville, Tenn.

Two groups of white rats, matched for age, sex, and genetic constitution, were subjected to diverse conditioning procedures in an electrified tambour-mounted cage. The 26 animals of one group escaped a shock if they responded sufficiently early to a light which preceded onset of the shock by 2 seconds. The 26 matched animals of the other group always received a shock, regardless of response to the light. Kymograph records for each animal were analyzed in terms of the number of trials, shocks, and anticipatory responses prior to achievement of 5 and 10 successive anticipatory responses.

The 2 groups did not differ significantly in number of trials required to reach these criteria. Animals which escaped shock, however, required significantly fewer shocks and made a significantly greater number of anticipatory responses than did those which received shock at every trial.

Vincent acquisition curves for anticipatory responses show a rapid initial rise followed by a slow one. Their shape is similar, regardless of the conditioning procedure—perhaps an argument for the essential similarity of the functions involved. The slope of the curve was greater for the achievement of the high than for the achievement of the low criterion of conditioning.

The Effects of Regularity of Stimulation in Conditioned-Response Learning. JAMES S. CALVIN, Yale University, New Haven, Conn.

Recent attempts to study effects of distribution of practice in conditioned-response learning have effected distribution by 2 means: (1) by lengthening or shortening the temporal interval between successive presentations of paired stimulations, keeping rate of stimulation regular; (2) by irregular stimulation, altering the average length of the temporal interval.

Both methods have yielded results indicating advantage of longer intervals for conditioning. Explaining these results in terms of distribution of practice overlooks the possible importance of regularity of stimulation; even objectively regular intervals may differ in apparent regularity to the subject.

The present experiment attempts a test of the regularity factor in the conditioning of an eyelid response. Three groups of 20 human subjects each were employed. Groups A and B received paired stimulations at regular intervals; the interval between successive presentations was 3⅓ seconds for Group A and 6⅔ seconds for Group B. Group C received paired stimulations at irregular intervals of either 3⅓ or 6⅔ seconds, in random order.

Results indicate superiority in conditioning for Group C (irregular) over Groups A and B (regular).

Implications for the following theories are discussed: perseveration; stimulation-maturation; temporal conditioning; negative attitude; work decrement; interference; boredom or monotony.

The Goal Gradient in a Circular Pathway. A. DUDLEY ROBERTS, University of Kentucky, Lexington, Ky.

Both goal and start have been emphasized in interpreting the goal gradient in learning. To test the influence of each, 12 white rats were electrically timed in swimming 6 successive quadrants of a circular tank for 9 trials.

In Group A each animal was started always from the same segment, thus passing through the entrance and goal segments before concluding the trial. In Group B a paired animal started each successive trial in different segments.

The effect of place cues is indicated by a marked greater slowing up among Group A on first passage through the goal segment, an effect decreasing in later trials. In contrast, Group B indicated an influence related to the start or to distance traversed. The distance cue was further indicated by the slowing up of this group in the final goal segment when allowed to traverse 2 additional segments on the ninth trial.

An explanation is offered in terms of learning as adjustment in relation to a variety of cues and by the tension-relieving effects of the progressive responses to the cues.

The Effect of Early Isolation of White Rats on Competition in Swimming. A. G. BAYROFF, University of North Carolina, Chapel Hill, N. C.

Two sets of white rats, one reared in individual cages, the other reared in groups, were forced, 1 at a time, to swim under water and against a current. The animals were then paired, each pair containing 1 animal reared in isolation with an equally fast socially reared animal. Each pair was made to swim, the animal reaching the exit first being allowed to escape into the air. The second animal was trapped under water for a short time before it was permitted to escape.

The results indicated that the number of solitarily reared victors was equal to the number of socially reared victors. In a small number of pairs neither member won consistently. It appears that in this experiment the nature of the early life is not the principal determiner of success in competition.

Other factors, such as weight, sex, etc., do not appear to be the principal determiners. Significant changes in swimming speed occurred, both victors and losers showing such changes.

A Direct Comparison of 3 Methods of "Delayed Response." JOHN T. COWLES, University of Alabama, University, Ala.

As a working hypothesis, the animal behavior problem which has been typically called "delayed response" was treated here as *successive pairs of discrimination trials,* the first of each pair being a training trial without immediate food reward, the second being a test of retention in a somewhat altered situation followed by food reward. An apparatus was devised which permitted the incorporation within 1 unit of any of several types of *training* procedure, yet preserved a relatively constant *test* procedure. The subjects were young albino rats, all of which were first given thorough training on a simple black-white discrimination in a Y-shaped box. Each subgroup of 30 animals was then given a different type of training trial, but the same type of test trial in each successive pair of trials. Various scores clearly indicated that the closer the response allowed in training trial approximated the response allowed at test after delay, the better the retention. The significance of the relation of pre-delay and post-delay responses is therefore emphasized—a factor which may account for the discrepancies in limits of retention of this species as shown by previous experimenters. These and other observations suggest the fruitfulness of this experimental treatment toward understanding "delayed response" as essentially a discrimination-learning and transfer problem and not a unique type of "symbolic" behavior demanding unusual "representative factors."

SECTION II

CHAIRMAN, KARL E. ZENER

Is Infant Behavior Appreciably Affected by Cultural Influences? WAYNE DENNIS, University of Virginia, University, Va.

In the tribal culture of the Hopi Indians, the infant receives a treatment which differs in many respects from that given to the white American child. The Hopi infant is subjected to restraint through the process of cradling; he is always in the presence of an adult or an older child; he nurses throughout the first year or longer; he receives no elimination training during the first year; and he receives a minimum of discipline, especially with respect to being warned against damage of household equipment and other aspects of the material culture.

The author's field notes and observational data show that in spite of these cultural differences, infants among the Hopi exhibit the same social and motor responses as do American infants. Positive social responses appear at the same age in both cultures, and shyness and fear of strangers likewise appear at the same age levels. Other reactions likewise are entirely comparable.

The bearing of these findings on the question of the locus of cultural effects in the individual life history will be discussed.

A Study of Conditioning in the Neonate. DELOS D. WICKENS and CAROL WICKENS, Ohio State University, Columbus, O.

Subjects were under-ten-day-old infants, born in Ohio State University Hospital. The conditioned stimulus was a buzzer; the unconditioned stimulus, a shock to the sole of the foot, resulting in a specific " withdrawal " movement. If any infant responded to the buzzer alone before the conditioning procedure, he was rejected. The experimental group (12 subjects) received paired stimulations of buzzer and shock, 12 per day for 3 consecutive days. At the end of day 3, the buzzer alone was sounded until extinction of any response. On day 4, the presence of spontaneous recovery was checked. The control group (12 subjects) received 12 shocks *alone* on 3 consecutive days. Responses to the buzzer alone were tested for in the same manner as in the experimental group. Results of the 2 groups were highly similar, both showing clear-cut responses to the buzzer alone at the end of the third day, as well as phenomena of " extinction " and " spontaneous recovery "—this, although the control group had never had a paired stimulation of buzzer and shock. These results make somewhat questionable previous claims for the presence of the usual type of conditioning in neonates and below.

Shorthand in the Education of a Deaf-born Child. MAX F. MEYER, University of Miami, Coral Gables, Fla.

Fifty years ago Alexander Graham Bell pointed out that, in order to learn to speak, a deaf child should have a phonetic guide other than that furnished by lipreading and by conventionally spelled printed words. Even to the person who already has a vocabulary, lipreading furnishes, at best, barely half the speech sounds. And English spelling is absurdly unphonetic. So he proposed that the speech teacher furnish the child for its guidance phonetically written words of the English language. Reading words in English spelling would come much later, just as it comes long after the normal child has acquired a speech vocabulary. For very good reasons Bell proposed that the phonetic symbols used for writing should be shorthand symbols rather than any other kind of phonetic "letters." Bell tried this educational experiment for 1 year. But being unable to find a teacher patient enough to continue it, his experiment remained inconclusive. Forty years later I became interested in this problem of educational psychology. And I have been working on it continuously for these 10 years. My conclusions are that Bell's idea is entirely sound; the educational method is entirely feasible; and it is a pity that the routiniers making up the rank and file of the teachers of the deaf have all these years been entirely impervious to Bell's suggestions.

The Development of Animistic Concepts in the Child. R. W. RUSSELL, University of Virginia, University, Va.

Interest in the experimental study of child animism has received considerable impetus from the publications of Piaget. Russell and Dennis have described a standardized method which makes it possible to classify children into the 4 stages of animism discovered by Piaget and, by so doing, permits an objective study of the development of animism. This method has been employed in studying 774 children, 361 of whom lived in a large northern city, 200 in a less thickly populated southern "suburban" area, and 213 in a southern "rural" district.

A consideration of the data seems to warrant the following summary of results: (1) It is possible to classify individuals into the stages of animism suggested by Piaget; (2) it is probable that individuals pass sequentially through the series of concept stages with increasing mental and chronological age; (3) although the fundamental validity of Piaget's classification of concept stages has been proven, the data have revealed the impossibility of limiting the age range of the stages as Piaget has attempted to do; (4) the development of animistic concepts is very nearly the same for all 3 experimental groups, despite their varied geographical locations and socioeconomic status; and (5) there are no significant differences in the development of the animistic stages between the male and female subjects in any of the 3 groups.

A Study of the Adjustment Behavior of Elementary School Children. WILLIAM M. HINTON, Washington and Lee University, Lexington, Va.

Two methods of measurement were utilized: (1) an objective check of the child's behavior under everyday frustrating conditions, based on ratings, and (2) a record of his reactions to experimentally controlled thwarting situations. Eighty-seven children were rated, and 51 of the rated group served as subjects for the experiments. An analysis of the results shows in part: (1) Marked individual differences exist in the adjustive tendencies of elementary school children; (2) the prevalence of "regressive" tendencies as compared to other types of adjustment is indicated; (3) no difference is shown between boys and girls in "subterfuge" behavior—the data suggest, but do not prove, that "regression" tendencies are more prevalent among girls and "tension" tendencies among boys; (4) no significant relationships are shown between mental age and any type of adjustment tendencies; (5) the ratings do not prove but suggest a decrease in "tension" and "subterfuge" and an increase in "regression" with each year in school. Finally, split-half and rerating correlations show that the rating method employed is a reliable technique for the investigation of adjustment tendencies, and that, when the results of the ratings are used as a criterion, the experiments are valid measures of the adjustment behavior tendencies of young children.

The Concept of Adolescence. RICHARD T. SOLLENBERGER, Yale University, New Haven, Conn.

Evidence is presented to further the hypothesis that adolescent behavior is a biosociological phenomenon and that the classical term, "adolescence," must be redefined as a behavior pattern rather than as a specific age-grouping in the life span of the individual.

The rapid physical, physiological, and psychical changes that occur at pubescence necessitate a reorientation and an adjustment of the individual acquiring adult capacities. "Adolescence" is an adjective descriptive of the manner of adjusting which deviates from the adult norms of behavior. The appearance and the duration of adolescent behavior are dependent upon the culture in which it is observed and upon the age of the pubescent individual.

The Social Adjustment of the Under-age High School Pupil and College Student. MITCHELL DREESE, George Washington University, Washington, D. C.

The purpose of this investigation is to determine the degree to which acceleration in high school and college results in social maladjustment.

The under-age pupils in 2 large Maryland high schools adjacent to Washington, D. C., were compared with the normal and over-age pupils on the following factors: (1) score on the Symonds' Adjustment Questionnaire, (2) membership in clubs, (3) elective offices held, and (4) teacher ratings of social adjustment.

An analysis was also made of the relationship between physical size and social adjustment of the under-age pupils.

A subsequent study of the under-age students in the George Washington University was then made to determine whether, in their opinion, they were handicapped by their chronological immaturity.

The data indicate that the under-age pupils in high school excel the normal and over-age pupils on all 4 criteria. Preliminary tabulations of the college data suggest that approximately 75% of the under-age students are not cognizant of any handicap attributable to their age.

Apparently, the present fear on the part of the school administrators concerning the acceleration of pupils is not substantiated by objective evidence.

FRIDAY AFTERNOON SESSION, APRIL 7

PHILOSOPHY

CHAIRMAN, GEORGE THOMAS

The Relation of the Aristotelian Categories to the Logic and the Metaphysics. K. K. BERRY, University of Virginia, University, Va.

In Aristotle's *Metaphysics* we find the assertion that one of the meanings of being is as it is divided into the categories. Certain concepts of the Metaphysics, derived from the natural science, are here held to be central: substance, becoming, potency, act, the 4 causes and fourfold analysis of change. Are the categories adequate to handle the operations of these metaphysical principles? The categories of substance, quality, quantity, and place bear the very names of the 4 kinds of change indicated; similarly in the case of potency and act. The apparently static character of the categories is due to the fact that they are used in predication. This can be shown by the fact that we may substitute the verb 'to become' for the verb 'to be,' following it with terms subsumed under each of the categories. The categories, therefore, are not 'deduced' from an analysis of the meanings of the verb 'to be' but rather from the world of change. Consequently, they bridge the gap between the properly dynamic nature of the natural sciences and the Metaphysics and the necessarily static, because conceptual and abstract, character of the logic.

The Schema of Levels in Progression. FREDERICK W. MEIER, Louisiana State University, Baton Rouge, La.

Distinct from the familiar "subject-object problem" is a knowledge-situation which is to be conceived under the general problem: How does man attain knowledge of a thing in process? A misconception of the situation is evident when knowledge of a process and knowledge of an object *in statu quo* are taken separately, because thing and process constitute one object of inquiry or investigation. This peculiar object gives rise to a problem of levels in cognition theory and methodology. Familiar versions of scientific method are inadequate to the task undertaken. There is no evolutionary method of discovery. The basic conception of the subject matter under investigation, which is maintained throughout from the beginning, is the idea of development or progression from within. But comparisons and classifications of similarities and differences take place in cross-sectional sampling and analysis of whatever materials are available. The status of a cross-sectional sampling and analysis in a systematic treatment of anything in progression is that of a plane set from a criticological standpoint, *as if* the subject matter under investigation were timeless and static. The schema as a whole represents both quantitative and qualitative differences in the same subject matter, maintaining degrees of differentiation as continuous throughout, while levels differ from each other qualitatively. A hierarchical arrangement of levels is necessary to represent the whole as progressing in some direction.

The Field and Method of the Philosophy of Science. C. K. DAVENPORT, University of Virginia, University, Va.

In attacking the general problem of the definition and scope of the subject I shall first consider some confusions which seem to have lead to serious misunderstandings of the function of the Philosophy of Science (confusions which have lead to attempts to identify it with the History of Science, with Cosmology, with Logic, with Mathematics, or with any of the special branches of science, *i.e.* with Science itself).

Then I shall propose an interpretation of what I believe to be its positive function as a discipline and as an organized program of inquiry among many subject matters. This will involve the question of whether metaphysical first principles come at the beginning or the end of scientific inquiry. Finally, I shall suggest the possible results of such a program in a concrete case.

Metaphysics and the Scientific Method. GEORGE TODD KALIF, 1441 Calhoun Street, New Orleans, La.

The success of the scientific method and the tendency of metaphysics to facility of explanation are understandable bases for emphasis upon method and for consciousness of the limitations of metaphysics for the purposes of science. Nevertheless, methodical obscurantism emerges from overemphasis on the routine which results from crystallization of method. This is most clearly seen in the setting up of scientific method as determining subject matter as well as meaningfulness in philosophy. Scientific method becomes constitutive and inflexible, thus becoming impervious to the influence of new ideas. It, then, represents a grasp at dogmatic finality. History, however, teaches that truth is approached asymptotically. In this process, metaphysics plays a rôle, for it is a reaching-out for broader and more general ideas. It represents the urge toward a more comprehensive vision of the universe, the urge toward greater values in new experience. Hence, it outstrips method, which is of itself prone to routine and degeneration; but metaphysics represents one phase of thought, the speculative, and method another phase, the practical. The two work together, metaphysics vitalizing method, and method determining the extent of the pragmatic value of metaphysics. This is a coöperative enterprise in the achievement of a more satisfying experience.

Examination of a Logical Positivist. PETER A. CARMICHAEL, Louisiana State University, Baton Rouge, La.

The views expressed in the book by Alfred J. Ayer, entitled *Language, Truth and Logic,* are the subject of this examination. Two of these views are especially considered: the first, that metaphysics is totally without meaning and that it is due to loose thinking—more especially to failure to understand grammar; the second, that value judgments are neither true nor false, but are only wishes or commands. Why do the propositions of logic and mathematics have binding force, and how is the regularity of natural phenomena explained, if metaphysical ground and principle are repudiated? Mr. Ayer says the explanation is found in our definitions, which, in turn, are due to convention. Here, it is argued that convention wholly fails to explain, since it only supplies a substitute set of data without accounting for the rigor of definitions. With regard to value judgments, it is similarly argued that they require a ground and that the phenomenalism of Mr. Ayer fails to supply this.

PSYCHOLOGY

SECTION I

CHAIRMAN, A. G. BAYROFF

Proprioception and Vibratory Sensitivity. JOSEPH WEITZ, University of Virginia, University, Va.

Although the receptor mechanism for vibratory sensations has, in the main, been relegated to 1 of 3 categories, pressure receptors, bones, and periosteum, or special vibratory receptors, another possibility has been suggested by Cohen and Lindley. They state: " It may be true that vibration and proprioception are merely quantitatively different aspects of the same sense-modality . . . " They deduce this from the alleged fact that vibratory thresholds are raised when kinesthetic strains are present, thus diminishing the possibility of stimulating the same fibers with vibratory impacts.

In the present study there is an attempt to investigate the influence of various intensities of kinesthetic sensation on vibratory thresholds with rigorous control applied to the stimulating conditions. It would seem that any changes in threshold, ensuing from the presence of kinesthetic sensations, are explicable on the basis of their serving as distracting, attention-demanding stimuli in competition with the vibratory stimulus. There appears, therefore, no need to assume common mediation for vibratory and kinesthetic sensations.

Vibration Spots and Their Underlying Tissue—A Preliminary Report. B. VON HALLER GILMER, Carnegie Institute of Technology, Pittsburgh, Pa.

Of the enormous literature concerned with the sensitivity of the body to vibratory stimulation, no consideration, other than speculative, has been given to the possible receptors of vibration. Most recent investigations have concurred with the early views of Von Frey that vibratory sensibility is mediated by the cutaneous pressure receptors, but of these we know little. The evidence from cutaneous excision experiments, both direct and indirect, designed to determine the receptors underlying temperature, pain, and touch " spots," has, on the whole, been inconclusive. Consideration is given to these investigations in light of the present preliminary report, which describes the methods used in locating and excising vibration spots, together with an histological description of the tissue underlying them.

Configurative Effects in Binocular Interdependence. S. RAINS WALLACE, JR., Tulane University, New Orleans, La.

In previous papers the author has reported a number of phenomena which indicate the existence of a dynamic interdependence in the binocular pathways. One of these phenomena also demonstrates that the interdependence effect is a function of changes in the configuration of the visual field and is independent of brightness changes or of shifts in accommodation, convergence, or fixation. Thus, the problem is raised concerning what difference between the underlying neural processes may be invoked to explain this functional difference between configured and unconfigured fields.

A series of experiments which indicate that configuration is mediated by a different neural mechanism than brightness or, perhaps, color is discussed. The work of other experimenters who have reached similar conclusions is reviewed, and a simple stereoscopic experiment which suggests that present concepts of form are inadequate is demonstrated.

The Effect of Temperature Upon Typical Potential Changes, or C. E. S. in the Spinal Cord of the Frog. CECILE BOLTON FINLEY, University of Virginia, University, Va.

The work of Matthews on cord potentials (April, 1938) is briefly reviewed. An investigation is reported of the effects of temperature upon the normal wave-form of the potential change, found by Matthews to be set up whenever a single volley of impulses enters the cord and to spread to the dorsal roots where it can conveniently be measured. Photographs are reproduced of the potential changes taking place in the cord of *Rana esculenta* at various temperature levels, showing the variations which occur in the latency of the change and in its rising and declining phases. Curves relating temperature with latency, with time to crest, and with time for 25% and 90% of the change to subside are presented, exhibiting in each instance a decrease in time with rising temperature, but a difference in the form of the first named (latency-temperature curves) and the latter curves. Temperature coefficients presented also exhibit differences, and the conclusion is reached that the processes responsible for the initial delay and the subsequent development of the potential changes, or central excitatory state, in the cord are different in nature, the former due perhaps to a single chemical process and the latter to more complex processes.

A Study of the Factors Determining Discrimination of Size by the White Rat. ROBERT M. FLORY, University of Virginia, University, Va.

In a number of studies, attempts have been made to show that discrimination between paired stimuli which differ only in degree is based upon either relation or absolute cues. Recently, Spence has attempted to explain such behavior by the use of conditioning principles.

In this experiment, 18 white rats were trained to go to either the larger or the smaller of 2 white squares when presented together. They were then presented with other pairs which bore the same ratio to each other as the training pair, but which differed in absolute size.

It was found that the tendency to respond on the test in a manner consonant with the training (*i.e.* to the larger or to the smaller) is a function of the degree of similarity between the 2 pairs. A theoretical interpretation in terms of conditioning principles is given.

SECTION II

CHAIRMAN, WILLIAM A. BROWNELL

A Psychometric Study of Meaning. CHARLES I. MOSIER, University of Florida, Gainesville, Fla.

Three hundred adjectives, expressive of judgments of favorableness or unfavorableness, were selected from Thorndike's *Word List* and presented to 150 college students for judgment as to degree of favorableness or unfavorableness expressed. Judgments were made under the instructions appropriate to the method of Equal-appearing Intervals and data treated by the methods of Equal-appearing and Successive Intervals. The data, in general, conform to the *as if* hypothesis that meaning is composed of 2 components, 1 fixed for all subjects and 1 which varies in the manner of random errors from individual to individual. The distributions of responses for nearly all words project Gaussian distributions on the same continuum.

Analyses of meaning and of the act of judgment are presented. Certain unanticipated results indicate that favorableness and unfavorableness are qualitatively distinct rather than quantitative variations along a single continuum.

The Effect of Partialling on the Thurstone Factor Method. W. J. E. Crissy, University of North Carolina, Chapel Hill, N. C.

Five variables with known factor loadings (obtained in a factorial analysis elsewhere reported *) were refactorized by the same technique as used in the original problem, for testing the property of "invariancy of factor loadings." Also, a factorization of 4 of these variables was made, the fifth being partialled out of each of the others prior to the application of the factorial procedure. These 2 factorizations, one with the original variables unaltered, the other with age partialled out of the original variables, were made with the hope of showing empirically, in an admittedly limited problem, (a) "invariancy of factor loadings" and (b) the effect of partialling on factor loadings. Results obtained indicate (a) invariancy of the original loadings and (b) no significant effect of partialling on the original loadings. As an addendum, the relationships herein treated were compared with results of the same sort from a Kelley factorization of the same data.

Factor Analysis of Music Tests. Raleigh M. Drake, Wesleyan College, Macon, Ga.

All tests having reliability coefficients of over .31 were selected from a total of 20 music tests for the purpose of factor analysis. The Spearman tetrad-difference technique was employed. Five of the tests were from the Seashore battery and consisted of Pitch, Rhythm, Intensity, Time, and Tonal Memory; 1 test, Tonal Movement, was from the Kwalwasser-Dykema battery; and 2 were constructed by the author and were called Musical Memory and Retentivity. One common factor for all 8 tests was found, but group, or specific, factors to the number of 3 and involving 4 of the tests are shown to be present. Two other minor group factors exist, but are considered to be of slight significance. A suggestion is made as to the nature of the common factor.

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* Richmond meetings of the A. A. A. S., December, 1938.

The battery is also analyzed after breaking it down into 2 groups. One group involves only the 5 Seashore tests, and the other group includes only the 3 tests which have satisfactory validity coefficients.

An Approximation Method for Obtaining a Maximized Multiple Criterion. ROBERT J. WHERRY, University of North Carolina, Chapel Hill, N. C.

Horst and, later, Edgerton and Kolbe developed methods for obtaining maximized multiple criteria. Based upon different assumptions and differently developed, the methods resulted in an identical formula. An approximation method was also given by Horst, due to the extreme tediousness of the method. Edgerton and Kolbe, however, showed that for their example the Horst approximation technique yielded results quite at variance with the actual weights.

The present paper gives a derivation of a new approximation technique. It is shown that separate formulae are needed, depending upon the sign of the average value of the intercorrelation coefficients. The formulae are shown to give adequate approximations for both the Horst and the Edgerton and Kolbe examples.

Comparison of Measures of Tendency-to-Continue. DOROTHY RETHLINGSHAFER, University of North Carolina, Chapel Hill, N. C.

Fifty-eight children were given 11 tests of tendency-to-continue as measured by behavior following interruption, and these tests were then evaluated by various statistical criteria. By applying the Thurstone multiple factor technique to the intercorrelations between the tests, 2 centroid factors were found sufficient to account for the size of the intercorrelations. The tests were therefore evaluated in terms of the communality coefficients of the first largest factor after rotation. One test was selected as representing the best measure of tendency-to-continue an activity as indicated by behavior following interruption.

On the basis of this analysis with the 11 tests used with children, 2 other tests measuring behavior following interruption were made and given to college subjects. Added to these measures of tendency-to-continue were 20 other measures of resistance to change, 9 being tests of perseveration, 9 of persistence, and 2 of attitudes. An intelligence test was also included in the battery. A preliminary report is given on the latter study with college subjects.

SATURDAY MORNING SESSION, APRIL 8

PHILOSOPHY AND PSYCHOLOGY

CHAIRMAN, FRANK A. GELDARD

Plato and the Dilemma of Literary Criticism. IREDELL JENKINS, Tulane University, New Orleans, La.

Plato censored art. In doing so, the heart of his argument must have been this: Art which is directly imitative must be so indirectly; if a situation is represented, the passions proper to this situation must be conveyed; art, then, must have effects on character and repercussions in action; some of these are good, some bad; so art must be censored.

We would reject Plato's conclusion theoretically, and we do reject it in practice. But there is less agreement as to what to do with the premises. The modern attitude is summed up in the legally accepted doctrine that art and literature cannot be obscene. This establishes practice, but it hardly establishes theory; rather, it exposes a difficulty and a question. The difficulty is that only by a minority will works of art be seen as such, and so purely; then, what to do about the majority, to whom they will be obscene? The question is: How can any work be directly imitative and not also indirectly so?

This paper attempts to justify both Plato's sponsorship and modern rejection of censorship in terms of different social conditions and to expose the imitative dilemma by a consideration of the act of appreciation.

The Place of Philosophy in the Changing Curriculum. MARTEN TEN HOOR, Tulane University, New Orleans, La.

(1) The first task of this essay is to establish, to the extent to which this is possible and permissible, stable meanings for the terms "philosophy" and "liberal arts curriculum"; the second task is to establish a relation between them which is not only justifiable in reason but realizable in educational practice. All the subjects of the traditional curriculum can, within limits, be arranged in a logical hierarchy, in accordance with the principle of degree of inclusiveness of concepts and principles. The philosophical disciplines can be located in this scheme with considerable exactness and their logical relationship to other subjects and groups of subjects exhibited, *e.g.*

ethics as basic to the social sciences, aesthetics to the arts, metaphysics as most inclusive from the standpoint of content, and logic as most basic from the standpoint of method. (2) No curriculum can completely " realize " this scheme, partly because it is an ideal one and partly because of the influence of ulterior motives in curricular organization. Nevertheless, the scheme is useful as a guide. Departments of philosophy might well make some effort to apply this principle in determining the order in which philosophical disciplines should be pursued by students specializing in philosophy. (3) Contemporary experiments in curricular reorganization can be evaluated in the light of this scheme. Many curricular changes seem to be more apparent than real. Of the 3 variables, student, system, and instructor, the system is the least important, for the inquiring mind needs no detailed directions for the pursuit of truth. Yet some recent changes—for example, from the departmental to the divisional scheme of curricular organization—are useful because they emphasize the logical relationship of the philosophical disciplines to other subjects.

Are There Any Individuals? HERBERT SANBORN, Vanderbilt University, Nashville, Tenn.

In the perennial discussions for and against the reality of Universals, the reality of individuals has regularly been accepted without scruple, in spite of trenchant assertions, like that of Duns Scotus, that the individual is an unknowable. *Ratio singularitatis frustra quaeritur;* the *quidditas* may be caught in our intellectual nets, but the *haecceitas* slips through the meshes. Doubtless, the individual has often been conceived quite as abstractly as the infamous universal, with neither idol of the forum capable of existence. The attempt of typical idealism to avoid this fallacy of abstraction does not, however, in its concretion of universal and individual, provide apparently for more than 1 individual; the rest disappear in, or into, the maw of the universal. The individual of vitalism is also inadequate.

Without attempting to settle whether, or how, the existence of individuals can be established or whether they be one or many, it is argued here from logical and psychological considerations that there are no individuals in one common acceptation of this term, while real individuals must be conceived, as with Lotze, Bowne, and the late Wilhelm Stern, under the form of " person," without its being necessary to accept the special application and extension of the term in the systems mentioned.

Rival Principles of Causal Explanation in Psychology. H. M. JOHNSON, Tulane University, New Orleans, La.

Many doctrines about causality presuppose some scientifically useless propositions. One is that if one event-complex, *P,* causes another event-complex, *Q,* then *P* temporally precedes *Q*. But given that *Q* cannot fail if *P* occurs, then *P* is sufficient for *Q,* and *Q* is necessary to *P*. Perhaps we can point to no actual instance of such an interdependence. But if Nature is orderly, then any event-complex of one instant is related to others in the same instant and also to those which occurred both before and after. These correlations, if completely stated, would express the laws of efficient causation, mnemic causation, and telic causation, respectively. Practically, one can regard *Q* as a telic cause of *P* only after both events have occurred. Many psychologists promise to use efficient causation only, but actually refer to unformulated laws of mnemic causation to explain association and learning and of telic causation to explain volitional behavior. But efficient causation has some serious limitations that do not arise out of our ignorance. We should try to formulate our laws of mnemic and telic causation accurately and use them openly without shame. They supplement, without displacing, the laws of efficient causation.

Psychology as a Natural Science. W. S. WEEDON, University of Virginia, University, Va.

The question of the status of present-day psychology with respect to natural science is raised primarily by 3 features of its approach: (1) the acceptance of certain branches of physical science as yielding results directly usable by psychology, (2) the wide-spread adoption of the experimental techniques, and (3) the employment of a set of basic conceptions analogous to those of natural science. The problem is thus posed: Whether present-day psychology may, in fact, be regarded as a natural science, and whether certain fundamental difficulties inherent in contemporary physical science are actually to be discovered in psychology.

It is beyond the scope of this paper to treat the problem in detail, and attention is therefore restricted to the organs of sense as physical objects. The special mode of functioning which these objects exhibit must be related to their character as objects discriminated within Nature. The difficulty of stating this relationship has certain analogues in the case of physics, and its solution (along one of several possible lines) vitally affects the question whether psychology is, in fact, a part of the science of Nature.

SATURDAY AFTERNOON SESSION, APRIL 8

PHILOSOPHY

CHAIRMAN, STEPHEN A. EMERY

Motse and Hobbes. MARJORIE S. HARRIS, Randolph-Macon Woman's College, Lynchburg, Va.

Motse lived in a time of political and social disintegration. His own humanitarianism prompted him to attempt to set right a time so out of joint. But the very urgency for remedial measures blinded him to the fundamental cause of the ills he sought to cure and defined for him very narrowly that *eudaemonia* it was his aim to foster. However, Motse was not the last man to defeat his real goal by an improvident practicality.

Our examination of Motse's measures for bringing about security will be facilitated by comparing his program with that of Hobbes. Both plans were based on the assumption that man is, by nature, egoistic. But Hobbes thought the sovereign alone could establish peace and prosperity, whereas Motse saw such an achievement as a coöperative enterprise. Yet each program would have failed if instituted. The rewards that were to stimulate endeavor could not have accrued to the people. The way of life designated for them could not furnish a satisfactory substitute for the unattained prizes.

Some Important Aspects of Individualism and Collectivism. WILLIAM E. FORT, JR., Winthrop College, Rock Hill, S. C.

The purpose of this paper is to bring out some of the important implicit and explicit characteristics that distinguish individualism from collectivism. There has been much confusion in the minds of both the educated and the uneducated concerning this subject. In an attempt to clarify this topic it will be useful to contrast these 2 opposed types of economic systems by setting up 2 hypothetical figures (Absolute Anarchism and Absolute Collectivism) in which the characteristics of each order are accentuated. The systems are then contrasted with reference to freedom of individuals, ownership of goods, coöperation, centralization, war and social coercion, and the definition or general conception of the State.

Soul as Process in the Philosophy of Plato. ANNA FORBES LIDDELL, Florida State College for Women, Tallahassee, Fla.

If literal translation of the Vision of Er indicates that Plato regarded the soul as an objective entity, the figure of the soul in the *Phaedrus* shows the absurdity of such a notion. In the *Cratylus,* soul is described as that which holds and carries and gives life and motion to the entire nature of the body. The body is the instrument of the soul, and the two are united in one consciousness. Sensation stimulates the soul to reflection. Recurrence of similar sensations gives rise to speculation, in which soul exercises its peculiar function, that of recognizing Universals. It is questionable whether soul could know Universals unless first stimulated by sense impressions. The body seems to be the necessary instrument of the soul. Every bodily awareness moves the soul to feel pleasure or pain. The truly pleasant is the natural, which is the harmonious. Soul finds permanent enjoyment in exercising its own function.

Naturally, soul undertakes to control bodily activity so as to secure pleasure and avoid pain, more especially to cultivate its own activity. Pleasure and pain furnish soul with means and method for self-development. It is the faculty for enjoying life which lends itself to education; one pleasure is replaced by a more enduring pleasure, and the final goal is to attain the full satisfaction of a perfect life.

Philosophical Problems in the Interpretation of Indian and Chinese Doctrines. FRIEDRICH SPIEGELBERG, Columbia University, New York City.

The difficulties in trying to come to a right understanding of Eastern doctrines are only partly of a philological nature. They are found ultimately in the philosophical and psychological approach. Not only the ideas, but the very categories, are different in Orient and Occident. The study of foreign doctrines can show that the schematism of a categorical pattern, as, for instance, that of Aristotle or Kant, is only of relative validity, being valid within a specific anthropologically limited structure of consciousness.

These philosophical problems become even more complex because we are wont to see a contrast in any relation of our own mentality to another. Thus, ideas, rejected in Western culture, and projected into the picture of our shadow, "The East," increase the already existing difficulties of true interpretation.

This can be proved by an analysis of a number of typical misinterpretations of Indian and Chinese doctrines: *e.g.* we want the Indian "Maya" to be what "illusion" means to us; we usually translate the Chinese "Wu-wei" by "non-action." In both cases, as in many others, we have not achieved a real understanding of the Eastern mind. The difference in the oriental conception of time as a wheel, and the occidental, as a line, is one of the basic impediments which have to be overcome by strict analysis before we can even start a comparative study of Indian and Chinese doctrines.

The Causal Dilemma in the Mind-Body Problem. D. M. ALLAN, Hampden-Sydney College, Hampden-Sydney, Va.

The current revival of epiphenomenalism among physiological psychologists contrasts sharply with the tendency of philosophers of widely differing schools to defend interactionism in principle. A critical application of the inductive causal methods to the psychophysical facts gives no support to the elimination of conscious processes from a dynamical explanation of behavior. The persisting objections of psychologists to the dualistic and mystical implications of interactionism appear to rest upon: (1) eighteenth-century ideas of causal relations; (2) confidence in the future of neurological explanation; (3) methodological difficulties in the way of treating consciousness dynamically. Unless we continue to shield conscious processes in esoteric manner from the causal logic, there seems no other alternative than to admit that they are co-determinants of behavior and interpret this determination in terms of an organic-dynamistic conception of causality. The doctrine of levels has furnished us with a new setting rather than a solution to this problem. The interaction of levels seems a far more reasonable hypothesis than a multiparallelism of levels.

PSYCHOLOGY

SECTION I

CHAIRMAN, HELGE LUNDHOLM

Psychological and Sociological Factors Determining Man-caused Forest Fires. JOHN P. SHEA, U. S. Forestry Service; JAMES W. CURTIS, University of Kentucky; and HAROLD F. KAUFMAN, University of Missouri.

The purpose of this paper is to report 2 investigations in social psychology that were made, each with a different approach, toward a common end. The common objective was to determine the attitudes of forest residents toward the conservation of the forests, toward man-caused forest fires, and toward the regulations of the Forest Service as they affected the people in the forest areas investigated.

One study was made in Kentucky, the other in southern Missouri. The Kentucky investigation used a psychological approach.

A field study was made by James W. Curtis, graduate student in psychology at the University of Kentucky, who attempted to isolate and evaluate the psychological factors, favorable and unfavorable to forest conservation.

In Missouri a field study using a sociological approach was made attacking the same problem. It was conducted by Harold F. Kaufman, graduate student in sociology at the University of Missouri. Both researches were carried out during the Summer and Autumn of 1938.

In both areas it was found that attitudes, habits, folkways, and mores exist that give rise to man-caused forest fires which, in turn, seriously threaten the forest resources and the welfare of the people dependent on them for recreation and livelihood.

Criticisms of the 2 methods are made. Suggestions for remedial education on the part of the public are advanced and discussed.

Attitude-Interest Analysis of Florida State College for Women Students. DOROTHY ROSE DISHER, Florida State College for Women, Tallahassee, Fla.

The Terman-Miles Attitude-Interest Analysis Test, constructed to describe the masculine and feminine characteristics of groups, was administered to 556 students at Florida State College for Women. Taken as a whole, the group does not differ in masculinity-femininity reactions from a group of sophomore women students drawn from Stanford, Oregon, Washington, and Utah University populations. When the F. S. C. W. student body is grouped, on the basis of birthplace, into Florida natives, southern natives (exclusive of Florida), and northern natives, the Florida group, in contrast to the northern and western group, reveals a somewhat more feminine attitude and interest. This difference, while small, suggests that as the groups become internally more homogeneous for various cultural factors, they tend to draw apart with respect to the degree of femininity in attitudes and interests. The outstandingly feminine southern woman was not discovered in this study.

Values Scores in Predicting Vocational Interest Scores and College Grades. ELIZABETH DUFFY, Woman's College of the University of North Carolina; and W. J. E. CRISSY, University of North Carolina.

The Allport-Vernon Study of Values and the Strong Vocational Interest Blank for Women were administered to 108 freshmen at Sarah Lawrence College. Relationships between the 2 measures and between the values scores and college grades were investigated by correlational techniques and by the comparison of upper and lower thirds. In general, the 2 procedures yielded similar results, though certain exceptions are noted. There were a number of significant correlations, in the expected direction, between vocational interest scores and values scores. College grades showed a consistent, but slight, relationship to certain values scores. These values scores, when weighted and combined, correlated with grades as closely as did the scores on the American Council on Education Psychological Examination.

Attitudes of North Carolina College Students (Women) Toward the Negro. WILTON P. CHASE, Woman's College of the University of North Carolina, Greensboro, N. C.

Attitudes toward the Negro of 1027 women students from North Carolina in 3 North Carolina colleges were measured by Hinckley's Scale. Results show an increasing favorableness in attitude toward the Negro with decreasing percentage of Negroes in the total population, when analyzed on the basis of students' home counties. The attitude of these students is, on the average, more favorable than that of University of Alabama students from the South and less favorable than that of Ohio University students (data of Sims and Patrich). When the attitude of students from counties where 70% or more of the Negroes are farmers was compared with that of students from counties where less than 20% of the Negroes are farmers, no difference was found. Nor was there any difference between students from Piedmont and Coastal Plain counties. In both comparisons, counties with similar percentages of Negroes in the total population were used. Attitude toward the Negro of these college students seems to be associated with some, as yet unknown, factor attendant upon the percentage of Negroes in the total population.

Relative Influence of Commercial and Liberal Arts Curricula in Promoting Changes in Certain Attitudes. K. L. BARKLEY, Woman's College of the University of North Carolina, Greensboro, N. C.

During the first 6 weeks of the school year, 7 social attitude scales, edited by Thurstone, were given to representative groups of students from the 1-year commercial division and from the freshman class in a state-supported woman's college. The groups were retested in the spring with the same or with different forms of the scales. The changes in the median scores of the 2 groups were noted.

Since the commercial and regular students lived on the same campus, participated in common extracurricular activities, and were subject to similar community and developmental influences, there is some basis for concluding that differences in the degree of change in social attitude are due (in part) to the different influences of the 2 courses of study.

In some cases the changes were about equal in the 2 groups, notably in cases of attitudes toward evolution and law. In those cases where the freshman curriculum introduced material directly related to the attitude in question, the freshmen showed more marked changes, *e.g.* attitudes toward the Constitution and toward war.

The findings indicate that factors other than the curricula are of high significance in producing changes in attitudes.

The Relation of Personal History Data to College Success.

E. J. Asher and Florence E. Gray, University of Kentucky, Lexington, Ky.

The object of this study was to see if personal history data have any relation to college success, and if so, to see whether such data, when combined with ability test scores, would materially increase the accuracy of prediction of college success. The subjects used in the study were 200 students of both sexes from the College of Arts and Sciences, University of Kentucky. Using the Adkins-Toops item validation technique, the personal history items found on the Kentucky Personal History Blank were validated against a quality-quantity-perseverance measure of college success. The items which were found to have a definite relation to this criterion were scored to obtain a "personal history score" for each student.

This score was then correlated with the quality-quantity-perseverance criterion and with the more common quality-quantity ratio measure of college success. The general ability test scores of these students were likewise correlated with both of these measures of success. Multiple correlations were obtained between each of these criteria of college success and the combined personal history and general ability test scores. The results indicate that the personal history score materially increases the accuracy of general ability test prognosis of either criterion of college success. The personal history score is more closely related to the quality-quantity-perseverance criterion, while the general ability test score is more closely related to the more common quality-quantity ratio criterion.

SECTION II

CHAIRMAN, ROBERT J. WHERRY

An Experimental Study of Musical Meanings. BRANTLEY WATSON, Duke University, Durham, N. C.

The specific purpose of this investigation is twofold: (1) to determine with what consistency music can be classified according to various meanings at different age levels and (2) to devise an instrument whereby ability to discriminate between such meanings can be measured.

The general procedure followed in constructing the test of ability to discriminate between musical meanings was to devise a check-list of appropriate adjectives (abstract meanings) and to select musical recordings representative of each of the list items. Subjects could then indicate on the check-list the adjectives which best represented each selection as it was played. The test also required the subjects to rate each selection in terms of its musical attributes (pitch, volume, etc.).

In 2 preliminary tests the most suitable adjectives to be used were experimentally determined, and appropriate corresponding musical representations were objectively selected. In a third test the items selected were standardized on the basis of the judgments of expert musicians. It was thus possible to compare responses at different age levels with the expert criterion.

The data are analyzed from the standpoint of (1) general nature of musical meanings, (2) musical characteristics of different abstract meanings, (3) growth in ability to discriminate between musical meanings, and (4) general nature of the discriminative ability.

Oral Group Rational Learning Test. HENRY F. DICKENSON, Lincoln Memorial University, Harrogate, Tenn.

This paper, accompanied by charts and diagrams, describes and illustrates a new apparatus for measuring orally the rational learning ability of groups. A brief historical account of related previous attempts is given. The administration of the test to some 300-odd subjects, the establishment of tentative norms, and development of the test in general is handled in the paper. Probable usefulness, problems, and conclusions are presented.

Bodily Movements Accompanying Problem-solving Activity. ALAN D. GRINSTED, Louisiana State University, University, La.

Bodily movement of subjects solving mental problems was recorded by use of a new type of stabilimeter chair. These records indicate that there is a definite increase of movement at the points where the subject is changing to a new attack on the problem. Similar bodily shifts following the solution of the final problem, however, suggest that this movement is due to a relaxation of muscles which have been gradually brought under tension, rather than due to the assuming of new postural sets with each new attack.

Reminiscence in the Learning of a Punch-Board Maze. MARGARET ROBINS, University of North Carolina, Chapel Hill, N. C.

The purpose of this experiment was to determine the presence or absence of reminiscence in the learning of a punch-board maze. This particular experimental setup was selected as one which would minimize opportunities of review—a factor often used to explain the appearance of reminiscence in more verbal learning situations. The maze used was a copy of that employed by Tolman, Hall, and Bretnall in their study of the law of effect. Sixty-two students from elementary psychology laboratories served as subjects. They were divided into 2 groups: a control group who took successive trials on the maze and an experimental group who were interrupted for a period of 24 hours between the fourth and fifth trials. Control and experimental subjects were compared in terms of group scores and in terms of scores of those subjects who improved on the fifth trial. The comparisons were made both with respect to scores on trial 5 and to total scores. There was no evidence of reminiscence. The various methods of measuring reminiscence were discussed in their application to results of this experiment.

Sex Differences in Rate of Reading. JOSEPH E. MOORE, George Peabody College for Teachers, Nashville, Tenn.

In this study 1215 boys and 1518 girls, representing each consecutive class from the eighth grade in junior high school through the senior year in college, inclusive, were studied for sex differences in rate of reading. The subjects, drawn from 2 junior and 2 senior high schools and 6 colleges, consisted of approximately 100 individuals of each sex at each grade level.

The test used was the Rate of Comprehension Section of the Unit Scales of Aptitude, which contains 56 paragraphs of relatively equal difficulty. The subject is required to cross out the word which denies the thought in each paragraph.

The data were treated statistically. The results seem to show that the girls are consistently more rapid readers than the boys from the eighth grade through the sophomore year in college, inclusive. Boys at the junior and senior college levels surpassed the girls, but the difference between the mean reading scores was not statistically reliable. Statistically reliable differences, however, were found to exist in favor of the girls in the comparison of the reading scores of the total group of each sex and in 4 of the 9 comparisons at the grade or class levels.

SPECIAL SECTION IN EXTRA-SENSORY PERCEPTION

CHAIRMAN, J. B. RHINE

A Further Advance in Methods of Investigating Extra-Sensory Perception. J. G. PRATT, Duke University, Durham, N. C.

This report presents the results of an investigation undertaken to meet criticisms that have already been made and to anticipate others which are conceivable. The research is the joint work of Mr. J. L. Woodruff and the writer.

The experimental procedure involved the following safeguards against sensory cues and errors: (1) Both experimenters were present during all the tests. (2) The subjects were tested for their ability to guess cards completely screened from sight and handled entirely by Experimenter X. (3) The subject indicated his guesses by pointing in relation to 5 key symbols which were out of X's sight and unknown to him until after he had recorded the 25 cards at the end of the run. (4) Meanwhile, Experimenter Y, without seeing X's cards, recorded the key cards. (5) The 2 separate records were deposited at once in a locked box to be scored later by a third person. The record sheets were serially numbered in pairs and assigned for the purpose of the investigation so that every run could be clearly accounted for. (6) The 2 experimenters jointly checked each score from the cards and each entered the count in his personal record book. (7) The score from the record sheets was later compared with those of the experimenters and in case of a discrepancy the lower score was adopted.

Thirty-two subjects, chiefly undergraduate women, have been tested (October, 1938, through February, 1939) under these conditions to the extent of 2400 runs (60,000 trials). The results obtained are regarded as " inexplicable " by the hypotheses available, unless by that of an extra-sensory mode of perception.

PROCEEDINGS OF THE FOURTEENTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

J. P. GUILFORD, SECRETARY-TREASURER, UNIVERSITY OF NEBRASKA

The Fourteenth Annual Meeting of the Midwestern Psychological Association was held at the University of Nebraska, Lincoln, Nebraska, on Friday and Saturday, May 5 and 6, 1939. The known attendance, as indicated by the number who signed the register, was 362. A special feature of the meeting was the celebration, with the department of psychology at the University of Nebraska, of the fiftieth anniversary of the founding of its psychological laboratory by Harry Kirke Wolfe in 1889. The commemorative address was given by Walter B. Pillsbury at the annual banquet on Friday evening, followed by a vigorous address by the President, Edmund S. Conklin, on "The Present Status of Academic Psychology." A special lecture, sponsored by Psi Chi, was presented Saturday afternoon by Karl Bühler on "The Orientation of Organisms in Time and Space."

At the annual business meeting, held on Friday afternoon, 54 new members were elected, bringing the present membership to 468. It was voted to contribute the sum of $25.00 toward the support of the American Psychological Association Committee on Displaced Foreign Psychologists. The Treasurer's report showed an income of $373.10 for the year ending May 1, 1939, and a total expenditure of $382.58, leaving a cash balance of $215.71. The Council announced the next place of meeting as the University of Chicago, the last week of April or the first week of May, 1940, the exact dates to be decided later. The following newly elected officers of the Association were announced:

President, 1939–1940: J. P. Guilford, University of Nebraska.

Secretary-Treasurer, 1939–1942: Robert H. Seashore, Northwestern University.

Council Member, 1939–1942: N. R. F. Maier, University of Michigan.

PROGRAM

FRIDAY MORNING, MAY 5

SESSION A. GENERAL

CHAIRMAN, EDMUND S. CONKLIN, INDIANA UNIVERSITY

An Analysis of the Content of Recent Textbooks in General Psychology. EARLE E. EMME, Morningside College, Sioux City, Ia.

Twelve most recent textbooks in general psychology are analyzed from the standpoint of their emphasis upon 5 areas of psychological data. Present needs and interests of students and use of experimental data and findings are increasing in emphasis.

Evidence for a Science of Recreational Guidance. T. F. LENTZ, Washington University, St. Louis, Mo.

A presentation of evidence for a science of recreational guidance in the form of data indicating easily detectible and objectively demonstrable correlation between recreational preferences and practices and other personality symptoms. This evidence appears to open the way for development of a scientific basis for recreational guidance.

The Interlinkage of Hereditary Elements (Ancestral Occupations) With Present Artistic Aptitude. NORMAN C. MEIER, State University of Iowa, Iowa City, Ia.

From considerable data a theory is derived that present artistic aptitude is almost invariably interlinked with constitutional stock inheritance (ancestries having as usual occupations craftsman activities). The theory is based on the doctrine of orthogenesis. Other traits are involved and, taken together, are predictive of artistic competence.

Morgan's Canon and the Problem of the Nervous System. R. H. WATERS, University of Arkansas, Fayetteville, Ark.

The influence of Morgan's canon in the human field has culminated in an anthropomorphism of the nervous system of a more serious nature than the original sin of Romanes and his followers. Studies of the nervous system are of psychological importance only as they clarify the conditions of human behavior.

The Mental Fatiguability of Malnourished and of Mentally Defective Children. ARTHUR G. BILLS, University of Cincinnati, Cincinnati, O.

The first study, made with A. McGilliard, established that the greater than normal muscular and mental fatiguability of 15 malnourished children disappeared with recovery from malnourishment. The second, with H. Valentiner, demonstrated much greater fatiguability of 26 mentally deficient children, from continuous mental work, than of normal controls of equivalent age.

An Outline for the Synthesis of Viewpoints in Psychology. ROBERT H. SEASHORE and BENJAMIN BURACK, Northwestern University, Evanston, Ill.

A basic outline for analyzing schools of psychology also shows the systematic interrelations of the special fields of psychology. Contributions of schools represent, primarily, differences in interests, emphases, and approaches which are *supplementary* rather than opposing.

The Marriage Course as a Psychology Department Offering. LAIRD T. HITES, Central Y. M. C. A. College, Chicago, Ill.

Marriage is a personal relationship, psychological in nature. Success depends on mutual personality adjustments. Legal, biological, or sociological considerations, while important, are secondary. The course should discuss selection of a mate, courtship, marital adjustments, children, resolution of conflicts, and broken homes, in terms of personality relationships.

1. SYMPOSIUM ON LABORATORY REPORTS

CHAIRMAN, HULSEY CASON, UNIVERSITY OF WISCONSIN

SESSION B. CONDITIONED RESPONSE

CHAIRMAN, ELMER CULLER, UNIVERSITY OF ROCHESTER

Changes in General Behavior During the Conditioning of Dogs. IRVIN S. WOLF, Indiana University, Bloomington, Ind.

Records of the lifts of *all 4 feet* and of respiration were taken during the buzz-shock conditioning of the right hind foot. Amplitude, latency, and frequency figures show evidence of trial and error, hypotheses, and other learning phenomena.

The Pupillary Response Conditioned to Subliminal Auditory Stimuli: A Control Experiment. CHARLES ALFRED METZNER and LYNN E. BAKER, University of Wisconsin, Madison, Wis.

The pupillary response was conditioned to subliminal auditory stimuli. Differences in latency between the unconditioned and conditioned responses were obtained.

Bilateral Conditioning as an Indication of Side Preference. E. G. BRUNDAGE and W. N. KELLOGG, Indiana University, Bloomington, Ind.

Dogs were conditioned to lift *both hind feet simultaneously* to a buzz stimulus. Their weight was supported by a strap. Differences in amplitude, latency, and frequency in the CR's from the 2 body sides were demonstrated.

Pseudo-conditioned Responses in the Cat. H. F. HARLOW, University of Wisconsin, Madison, Wis.

Pseudo-conditioned responses (responses to a buzzer never before simultaneously presented with shock stimuli) were obtained in 11 out of 15 cats. The pseudo-conditioned responses were not specific to the test situation, were much weaker than the unconditioned responses, and were retained for a maximum period of 96 hours.

Factors in Human Salivary Conditioning. LOUISE FENGER JONES, University of Wisconsin, Madison, Wis.

Human salivary conditioning was investigated using Razran's absorbent cotton technique. The results show that the conditioned response training situation is more complex than has often been assumed and that a systematic variation of the external stimulating conditions may make possible some analysis of this complexity.

The Influence of Attitude on the Conditioned Eyelid Response. DAVID A. GRANT, University of Wisconsin, Madison, Wis.

Analysis of eyelid responses conditioned to a light stimulus showed that a group of subjects maintaining an attentive-expectant attitude toward the light was more rapidly conditioned than a passive group. Each of 16 subjects showed several distinct eyelid response patterns. Verbal reports indicated mounting expectation during each conditioning trial.

2. SYMPOSIUM ON CLINICAL PSYCHOLOGY

CHAIRMAN, GEORGE A. KELLY, FORT HAYS KANSAS STATE COLLEGE

3. SYMPOSIUM ON PERSONALITY

CHAIRMAN, H. H. REMMERS, PURDUE UNIVERSITY

FRIDAY AFTERNOON, MAY 5

SESSION A. ATTITUDES AND FEELINGS

CHAIRMAN, R. H. WATERS, UNIVERSITY OF ARKANSAS

Oral and Written Wishes of Rural and City School Children. GEORGE S. SPEER, Children's Service League, Springfield, Ill.

Seventy-six rural school children and 115 city school children were asked to write the 3 things they most wished would come true. Later, in a private interview, they were orally asked what they wished most. Wishes are classified by sex, age, rural and city school, oral and written, and variations noted.

The Relationship Between Certainty of Opinion and Amount of Self-estimated Information. STUART W. COOK, University of Minnesota, Minneapolis, Minn.; and EVELYN RASKIN, Randolph-Macon Woman's College, Lynchburg, Va.

The purpose of this investigation was to determine whether individuals who have definite opinions on controversial issues also believe themselves to possess crucial information about these same issues. The results show a definite correspondence between degree of certainty of opinion and amount of information an individual estimates himself to have.

Where Do We Get Our Attitudes? A. R. GILLILAND, Northwestern University, Evanston, Ill.

In order to discover how nearly attitudes of high school and college students are like those of their parents, 2 attitude scales, God and depression scales, were given to students and parents. Correlations ranged from .10 to .30, showing that home is only one, and a rather small, factor in formation of students' attitudes.

Some Factors Affecting the Change of Attitudes of College Freshmen. IRIS STEVENSON, Fort Hays Kansas State College, Hays, Kan.

Each of 3 elementary psychology classes was given 10 different proposed social actions, and the subjects were asked to rate their attitudes on a 7-point scale. Crystallization consisted of discussion of each proposition, and at the end of the week they were again tested to discover the change in attitude.

The Electrodermal Response to Advertising Copy. CHRISTIAN A. RUCKMICK, Minneapolis, Minn.

Responses of the sweat glands were recorded during a 3-second exposure of advertising copy. Several series of copy, run with 20 subjects, revealed an internal consistency of data and also gave results which tallied in a general way with choices obtained by the serial procedure of impression.

Time Required for 'Real' and for Hypothetical Choices. ROGER G. BARKER, University of Illinois, Urbana, Ill.

A study of preferences for edible liquids showed that choice times are shorter when the chosen liquid must be consumed than when the choice is hypothetical, are inversely related to the 'distance' between the liquids in the preference series, and are longer for undesired than for desired pairs of liquids.

Speed as a Determiner of Musical Mood. MELVIN G. RIGG, Oklahoma A. and M. College, Stillwater, Okla.

Five musical phrases were played, each at 6 different speeds, to 88 observers. The data indicate that the faster a phrase is played, the more joyful, or the less sorrowful, it tends to become.

1. SYMPOSIUM ON MENTAL HYGIENE

CHAIRMAN, H. MELTZER, PSYCHOLOGICAL SERVICE CENTER, ST. LOUIS

SESSION B. MENTAL DEVELOPMENT

CHAIRMAN, M. L. REYMERT, CHILD RESEARCH INSTITUTE, MOOSEHEART

Three Types of Fetal Activity. HELEN NEWBERY, Fels Research Institute, Yellow Springs, O.

Weekly 3-hour observations by pregnant mothers concerning fetal activity were recorded by polygraph. The mothers discriminated slow, quick, and rhythmic movements. Age tendencies and individual differences in activity are illustrated and certain influencing factors of maternal state discussed.

Electrical Accompaniments of Sleep Stages in the Neonate. LAWRENCE N. MARX, Ohio State University, Columbus, O.; and ISABELLE F. WAGNER, Princeton Radio Research Project, Princeton, N. J.

A parallel was found between previously established overt criteria of sleep depth and the gross electrical potential of 25 newborn infants. Electrical records were taken from a high gain amplifier recording continuously with a cathode-ray oscillograph. No significant age or sex differences were obtained.

The Effect of Enriched Educational Experiences Upon the Growth of Intelligence of Very Superior Children. BOYD ROWDEN MCCANDLESS, State University of Iowa, Iowa City, Ia.

Six very superior 4-year-olds (IQ-range, 125–165; mean, 139), given an enriched program, stood higher in IQ 6 months and 1 year after testing than originally almost exactly matched controls experiencing only preschool. Constructiveness of behavior and emotional stability seemed highly related to intelligence gain.

Domination and Social Integration in the Behavior of Kindergarten Children in an Experimental Play Situation. HAROLD H. ANDERSON, University of Illinois, Urbana, Ill.

This study of 49 kindergarten children in 3 groups followed experimental procedures developed for use in previous studies of children of preschool age. While showing certain departures from findings with preschool children, data offer only consistent evidence supporting 15 hypotheses and assumptions advanced in earlier studies.

The Comparative Rigidity of Different Chronological Age Groups With Equal Mental Ages. JACOB S. KOUNIN, State University of Iowa, Iowa City, Ia.

This study is an experimental analysis of Lewin's concept of rigidity as it applies to theories of age and of feeblemindedness. A hypotheticodeductive method was utilized, and experiments were designed to test 5 theorems. Three groups of subjects of equal mental ages but different chronological ages were studied.

Some Measurable Aspects of the Child's Home Environment. HORACE CHAMPNEY, Fels Research Institute, Yellow Springs, O.

Thirty variables of parent behavior toward the child were presented as graphic rating scales, adequately refined for skilled observers. Data show the variables to be measurable, relatively independent of age, and discriminative of characteristics of the parent environment thought to be important for personality development.

Significance of Month of Birth as Judged by Test Scores and Grades. E. L. CLARK, Northwestern University, Evanston, Ill.

High school rank, scholastic aptitude test scores, first-semester grades, and graduation from college were taken as indices of ability for over 3000 students. By an analysis of variance of many subgroups and intercorrelations between samples of 100, practically no evidence was found to favor one month above another.

2. SYMPOSIUM ON MOTIVATION

CHAIRMAN, PAUL THOMAS YOUNG, UNIVERSITY OF ILLINOIS

SESSION C. PERSONALITY

CHAIRMAN, ARTHUR G. BILLS, UNIVERSITY OF CINCINNATI

An Evaluation of Beck's Norms as Applied to Young Children. HARRIETT DUNMIRE, Wichita Child Guidance Center, Wichita, Kan.

In accordance with Beck's suggestion, an effort was made to find norms from 119 children between the ages of 5 and 11. FC, FY, Z, and P scores were lower. F+%, A%, M, S, CF, C, and R were fairly comparable. P failure decreased with increasing chronological age.

The Agreement Between Associates' Ratings and Self-Ratings of Personality. ELEROY L. STROMBERG, Oklahoma A. and M. College, Stillwater, Okla.

Reducing the self-ratings and associate-ratings of personality traits of college girls to the proportion of desirable responses out of the number of items attempted causes differences between such ratings to disappear. The results indicate that overestimation in the self-ratings of college women is not a common characteristic.

Baxter Group Test of Child Personality. EDNA DOROTHY BAXTER, Englewood Public Schools, Englewood, Col.

The Baxter Group Test of Child Personality is standardized for grades 1 to 8, inclusive. About 1200 tests were used in standardization, obtained from various parts of the country. Reliability, through the split-halves method, was .918. Validity, established through correlation with Individual Personality Tests, was .71. Grade norms were established.

The Home Adjustment Inventory: An Attitude Scale for Personnel Procedures. LYLE K. HENRY, Iowa State College, Ames, Ia.; and EARLE E. EMME, Morningside College, Sioux City, Ia.

The Home Adjustment Inventory measures the degree of affection of students for parents. The inventory was validated by item selection, by case studies, and by correlation with other inventories. Findings: Students have more affection for mother; women have more affection for parents; students who dislike parents are most frequently maladjusted.

Waking Suggestibility in Children—General or Specific? H. H. REMMERS, Purdue University, Lafayette, Ind.

An experiment with 118 children is reported, designed to determine the psychological concomitance of 2 tests of waking suggestibility used earlier by Aveling and Hargreaves, and further designed to determine the reliability of the tests according to a more defensible operational criterion than they used.

Psychoanalytic 'Catharsis' in a CR Situation. LYNN E. BAKER and CHARLES ALFRED METZNER, University of Wisconsin, Madison, Wis.

A psg response was conditioned to a nonconscious stimulus. In psychoanalytic terminology, we produced a system of ideas having emotional tone (complex), important parts of which (buzzer, psg "symptom") were in the unconscious. The experimental results indicate that the CR connection is changed by "catharsis" in this situation.

Home Setting as a Factor in the Problem of the Only vs. *the Non-only Child.* LELAND H. STOTT, University of Nebraska, Lincoln, Neb.

In the farm and town home situations "onliness" was not an important factor in personality adjustment. Slight differences in the former favored the *non-onlies* while in the latter they favored the *onlies*. The city *non-onlies* were significantly superior in "rationality," but the *onlies* definitely excelled in "adjustment," "independence," and "responsibility."

FRIDAY EVENING, MAY 5

ANNUAL DINNER

TOASTMASTER, ARTHUR G. BILLS, UNIVERSITY OF CINCINNATI

Address of Welcome: DEAN J. E. LEROSSIGNOL, University of Nebraska.

Anniversary Address: WALTER B. PILLSBURY, University of Michigan.

Subject: "The Nebraska Psychological Laboratory"

Presidential Address: EDMUND S. CONKLIN, Indiana University.

Subject: "The Status of Academic Psychology"

SATURDAY MORNING, MAY 6

SESSION A. SENSORY AND NERVOUS PHENOMENA

CHAIRMAN, C. A. RUCKMICK, MINNEAPOLIS, MINN.

Some Relationships Between Electrical Signs of Cortical and Peripheral Activity. JOHN M. HADLEY, State University of Iowa, Iowa City, Ia.

The electroencephalogram (motor and occipital), the electromyogram (*flexar digitarium commonalis*), and the electrocardiogram were simultaneously recorded from 20 adult subjects and the data analyzed to ascertain whether there were relationships between these measures as regards (1) individual differences and (2) moment-to-moment variations. The relations appear significant from the standpoint of individual differences.

The Relationship Between Brain Potentials and Personality. CHARLES E. HENRY, State University of Iowa, Iowa City, Ia.

Using 80 subjects, the per cent time alpha of the electroencephalogram and the "personality" rating of each individual were compared. The data indicate that *both* the dominant and rare alpha individuals tended to be accompanied by an extroverted personality. A slightly higher per cent time alpha was found for the introverted group.

The Effect of Insulin on the Nervous System. S. HOWARD BARTLEY, Washington University Medical School, St. Louis, Mo.

The essential changes in nervous activity under insulin were studied by examining the discharge in certain nerves *in vivo,* the spontaneous activity of the cerebral cortex, its response to retinal stimulation and to stimulation of the saphenous nerve. The conclusions outline the effects and compare them with those of strychnine.

An Analysis of Perceptible Series of Partials in a Vocal Sound. WILLIAM H. LICHTE, State University of Iowa, Iowa City, Ia.

The investigation concerned a short vocal sound, of constant pitch and loudness, during which partials 3 to 7, inclusive, became successively prominent perceptually. Analytical data showed that the way the sound was heard was determined by attentional factors (due to changes of partials in intensity), although masking was not negligible.

Sensory Discrimination for Alternating Current Stimuli. LOUIS D. GOODFELLOW, Northwestern University, Evanston, Ill.

A study of electrical stimulation of the skin (a part of the general research program attempting to extend the usefulness of the lower senses for the deaf and blind) reveals good differential sensitivity for both frequency and intensity. Utilizing it introduces other complications.

Determiners of Adaptation Time. JOHN P. NAFE, Washington University, St. Louis, Mo.

Variations in adaptation time are a function of the weight and area of the stimulus and of the amount and rate of the displacement of tissue.

Sensory Adaptation. K. S. WAGONER, Washington University, St. Louis, Mo.

Movement induced in tissue by the application of a weight is negatively accelerated as adaptation occurs, and the curves for the 2 phenomena are identical. These facts are interpreted to mean that sensory adaptation is not a fatigue phenomenon and is not due to any effect upon the end-organ or nerve, but is due to a failure of stimulation.

1. SYMPOSIUM ON CONSTANCY OF THE IQ

CHAIRMAN, BETH L. WELLMAN, STATE UNIVERSITY OF IOWA

SESSION B. APPLIED PSYCHOLOGY

CHAIRMAN, J. P. PORTER, OHIO UNIVERSITY

Differences Among Groups of Former Students at the University of Louisville. LAURENCE WHISLER, University of Louisville, Louisville, Ky.

'Active students,' 'graduates,' 'transfers out,' 'failed and dismissed,' 'probation and warning' withdrawals, and 'miscellaneous' withdrawals were compared on college entrance test scores, on range and average grades in high school and college, sex, fraternity, etc. Group differences in items taken singly and in combination were studied.

A Study of the Unit Cost of Obtaining Attitudes. JOHN W. HANCOCK, Purdue University, Lafayette, Ind.

In a study exploring the unit cost of obtaining attitudes toward the retail store, using 4 methods, it was found that the method of including 25 cents with the attitude instrument cost less on the whole and agreed with the other methods on the attitudes measured.

Evaluation of Factors Involved in Driver Training. A. R. LAUER and A. F. SILKETT, Iowa State College, Ames, Ia.

One hundred and twenty individuals, ranging in age from 11 to 65, were given both textbook and practical training in automobile driving. The findings suggest that an adequate knowledge of the car, the highway, and traffic regulations, as well as of proper driving habits, will greatly facilitate mastery of automobile driving.

The Diagnosis and Treatment of Orthoptic Difficulties in Relation to Reading. JANE HUTCHINSON, State University of Iowa, Iowa City, Ia.

This study shows how the Betts Visual Perception Tests and Gray Oral Reading Check Tests can be used as diagnostic measures in discovering orthoptic difficulties. The relationship of these difficulties to reading deficiencies is pointed out and 8 case studies made showing the effect of eye-muscle training on reading.

Legibility of Newsprint. Miles A. Tinker and Donald G. Paterson, University of Minnesota, Minneapolis, Minn.

In 6 studies of newsprint legibility (2956 subjects) line width, type size, leading, and type faces were investigated. Variation of type size and leading affected legibility least; type face affected it most. Results coincide only partly with current usage, but well with reader preference.

Reliability of Memory and Testimony for Visual Presentation. William E. Kendall, Northwestern University, Evanston, Ill.

A standardized test for memory of details (lantern slide) is analyzed for effects of familiarity, suggestion, confidence, and methods of report. Norms for college students are provided.

Ocular Photography: A Study of Ocular Patterns to Discover Characteristic Eye-Movements and Their Psychological Implications. H. F. Brandt, Drake University, Des Moines, Ia.

A symmetrical figure was designed to determine the right-left, top-bottom, and horizontal-vertical preference of eye-movements. Ocular patterns reveal a left, top, and horizontal preference with critical ratios statistically significant. Ocular patterns of students with high or low achievements are strikingly different.

2. Symposium on Contemporary Social Problems

CHAIRMAN, A. R. GILLILAND, NORTHWESTERN UNIVERSITY

Session C. Learning

CHAIRMAN, ARTHUR W. MELTON, UNIVERSITY OF MISSOURI

A Second Study of the Relative Effect of a Cerebral Lesion on Learning, Transfer, and Retention. Stanford C. Ericksen, University of Arkansas, Fayetteville, Ark.

The retarding effect of a cerebral lesion, approximately constant in locus and size, was found to vary directly with the difficulty of the task and inversely with the learning ability of the animals. An error analysis indicates some of the mechanisms producing differences in learning by normal and operated rats.

An Experimental Study of Oddity-Abstraction in the Monkey. JOHN A. BROMER, University of Wisconsin, Madison, Wis.

The problem of oddity-abstraction was investigated in a group of New- and Old-World Monkeys. Evidence obtained and checked by controls indicated that a constant factor making possible oddity-generalization was influential in determining the animals' responses.

Tool-Using in the Rhesus Monkey. PAUL H. SETTLAGE, University of Chicago, Chicago, Ill.

Three rhesus monkeys exhibited varying degrees of skill in the use of objects to obtain food situated beyond arm's reach. One showed almost spontaneous learning, and acquired the ability to retrieve tools and use various substitutes. The performances of the others showed rudimentary ability and meager insight.

Correlations Between Different Forms of Learning. HULSEY CASON, University of Wisconsin, Madison, Wis.

A survey has been made of several thousand raw correlations between different forms of learning and between learning and other test scores. The low correlations indicate much specificity in different forms of learning and in different cognitive functions.

Some Implications for Pupil Learning of Teachers' Classroom Questions. STEPHEN M. COREY and G. L. FAHEY, University of Wisconsin, Madison, Wis.

Verbatim records of some 30,000 questions, asked pupils by 6 high school teachers of English, history, and science, were analyzed to determine their bearing upon pupil learning activity. The relationships between type of question asked and various pupil and teacher characteristics were presented.

Interrelations of Vocabulary Skills: Commonest vs. *Multiple Meanings.* GEORGE D. LOVELL, Northwestern University, Evanston, Ill.

Previous vocabulary tests of *extensive* knowledge (measuring number of words for which a single commonest meaning is known) are here correlated with a new test of *intensive* knowledge (measuring number of different meanings known for the same list of words).

SATURDAY NOON, MAY 6

PSI CHI LUNCHEON

SATURDAY AFTERNOON, MAY 6

SESSION A. MENTAL TESTS

CHAIRMAN, MILES A. TINKER, UNIVERSITY OF MINNESOTA

A Laterality Experiment. W. H. MIKESELL and MARTIN PALMER, University of Wichita, Wichita, Kan.

Considering that there has been too much subjective judgment in the star-tracing experiment, the present problem aims to correct this difficulty by objectively recording error-time by an electric impulse counter and objectively recording electrically the orientation of error—that is, where, the number, and in what directions they occur.

Retest Results on the A. C. E. Psychological Examination. WILLIAM A. THOMSON, Carleton College, Northfield, Minn.

Retest results for the A. C. E. Psychological Examination gave a significant difference in the gross score mean and a very wide range of individual changes in gross scores. No significant difference was found when the 2 sets of scores were correlated with college grades. Individual raw score changes correlated very slightly with low scores.

A Study of the Influence of the Experiential Factor on Intelligence Test Scores. E. V. BOWERS, Marshall College, Huntington, W. Va.

A study of freshman psychological examinations showed that students with previous experience in taking objective tests made higher scores than those with no experience. It is suggested that further study may indicate a need for either a differential or a practice test prior to the regular psychological examination.

Test-Retest Reliabilities of Mental Ability, School Achievement, and Personality Traits. EDWARD E. CURTIS, HOWARD B. FULLER, and J. P. PORTER, Ohio University, Athens, O.

The Otis Classification, Bernreuter Personality, Pressey Interest-Attitude, and O. C. A. Rating Scale were used. Confirmatory *r*'s were found for ability and achievement for a 5-month and for 3- to 4-year intervals. Surprisingly high *r*'s, mean of .66, were found for personality traits for the shorter interval. The subjects were 130 ninth-grade students.

An Experimental Analysis of Speed and Level in Intelligence Testing. BRENT BAXTER, University of Minnesota, Minneapolis, Minn.

Relationships of speed, level, and power as measured by the Otis Self-Administering Test and as validated by 3 criteria of intelligence (honor-point ratio, Army Alpha, a College Aptitude Test) were studied. Group and individual testing results are compared.

An Objectified Practical Test for Clinical Psychologists. GRACE MUNSON, MILTON A. SAFFIR, and HELEN U. CHAMNESS, Bureau of Child Study, Chicago, Ill.

A half-day practical examination for clinical psychologists, in 11 parts, has been devised for the purpose of (1) testing actual techniques, (2) sampling clinical skills extensively, (3) including objectified evaluations of personal qualities, (4) conserving examination time, (5) equalizing difficulty for all subjects, (6) objectifying administration of examination, and (7) simplifying and objectifying scoring.

1. SYMPOSIUM ON FACTOR ANALYSIS

CHAIRMAN, H. A. BECHTOLDT, UNIVERSITY OF CHICAGO

SESSION B. SOCIAL AND POLITICAL ATTITUDES

CHAIRMAN, RAYMOND H. WHEELER, UNIVERSITY OF KANSAS

A Factor Analysis of a Questionnaire on Political Attitudes of Czechoslovak Students. EDMUND E. DUDEK, University of Nebraska, Lincoln, Neb.

A factor analysis of 300 students' scores (based on scale values) resulting from a questionnaire survey of Czechoslovak student political attitudes revealed 4 factors: Democratic, Nationalistic, Anarchistic, and Tolerance. A second analysis based upon tetrachoric *r*'s between selected items in the same questionnaire failed to show the same factors.

The Responses of 'Communists' and 'Non-Communists' to Concepts of State Forms. ALLEN L. EDWARDS, Northwestern University, Evanston, Ill.

The extent of uniformity (standardization) of responses to the terms, Democracy, Fascism, and Communism, was investigated. The findings seem to support the hypothesis that *definiteness* of stereotypes is related to degree of prejudice, but not to a factor of familiarity. The stereotype content for groups holding opposing attitudes, however, may differ markedly.

Children's Thinking About Nations and Races. H. MELTZER, Psychological Service Center, St. Louis, Mo.

The reactions of 1000 children, examined in 1934, and 382, examined in 1938, were studied for the purpose of gaining insights on the nature and development of nationality concepts and stereotypes in American children toward each and all of 21 nations.

The Relative Effectiveness of Cartoons and Editorials as Propaganda Media. ALBERT D. ANNIS, State University of Iowa, Iowa City, Ia.

The relative effectiveness of the cartoon and editorial in the modification of attitudes was studied experimentally, using militaristic and pacifistic cartoons and editorials equated as to subject matter, bias, and persuasiveness. The editorials were most effective with 382 college subjects and the cartoons with 183 high school subjects.

Shifts in Attitude Caused by Cartoon Caricatures. R. ASHER and S. S. SARGENT, Central Y. M. C. A. College, Chicago, Ill.

Subjects recorded their favorable or unfavorable attitudes toward a number of terms, such as ' Uncle Sam,' ' Labor,' and ' New Deal,' then reacted to cartoon caricatures of the same subject matter. Significant shifts in attitude generally occurred with strongly complimentary or uncomplimentary cartoons of subjects about which group opinion was neutral or divided.

2. SYMPOSIUM ON LEARNING

CHAIRMAN, DAEL L. WOLFLE, UNIVERSITY OF CHICAGO

3. SYMPOSIUM ON PRINCIPLES OF DEVELOPMENT

CHAIRMAN, HELEN L. KOCH, UNIVERSITY OF CHICAGO

Address: KARL BÜHLER, College of St. Scholastica, formerly of the University of Vienna.

Subject: " The Orientation of Organisms in Time and Space "

(Sponsored by the University of Nebraska Chapter of Psi Chi, National Honorary Society in Psychology.)

PROCEEDINGS OF THE WESTERN PSYCHOLOGICAL ASSOCIATION, STANFORD UNIVERSITY, CALIFORNIA, JUNE 23–24, 1939

FRANK C. DAVIS, SECRETARY, UNIVERSITY OF CALIFORNIA AT LOS ANGELES

The 1939 meeting of the Western Psychological Association took place at Stanford University on June 23 and 24 with approximately 150 persons in attendance. Following the annual banquet, held in the Stanford Union on the evening of June 23, President Knight Dunlap addressed those assembled on the topic, " The Historical Method in Psychology." The concluding session of the meeting, held on Saturday afternoon, June 24, was devoted to a symposium on the topic, " Contributions of Psychology to the Understanding of Social Issues." Ernest R. Hilgard was local chairman in charge of arrangements for the meeting.

Officers elected for the ensuing year, 1939–1940, were:

President: Howard R. Taylor, University of Oregon.

Vice-President: Frank C. Davis, University of California at Los Angeles.

Secretary-Treasurer: Ralph H. Gundlach, University of Washington.

The Association will meet in June, 1940, at the University of California at Los Angeles, Los Angeles, California.

PROGRAM

Age and Intelligence of Juvenile Delinquents. CECIL W. MANN, Claremont Colleges.

A group of 1731 juvenile delinquents, boys and girls, was measured on the New Revision of the Stanford-Binet Scale. The mean age of the group was found to be 14.47 years; the mean IQ, 84.45. No sex differences were found with respect to (a) mean age, (b) variability of age, (c) percentages in the various age groups,

(d) mean IQ (measured on Form L), (e) variability of IQ (measured on Form L), and (f) percentages in the various IQ levels.

Of the total group, 428 recidivists originally tested on the Stanford-Binet (1916 Revision) were retested at intervals of from 1 to 10 years on the New Revision (1937). Between the 2 measures there was a coefficient of correlation of .80. A group of 62 delinquents, tested and retested on Form L, yielded an r of .91.

An analysis of the variations of IQ over the retest period indicated that, of the 428 cases retested, changes in IQ of less than 3 points were recorded in 21% of the cases, while less than 7% had varied by more than 17 points.

Factors Correlated With Later Success or Failure of Intellectually Gifted Boys. LEWIS M. TERMAN and MELITA ODEN, Stanford University.

The report is based on a follow-up study of about 600 boys belonging to the California gifted group who earned Binet IQ's of 140 or higher in 1922. Degree of success of each was rated by 3 judges, who examined the records of accomplishment and classified the subjects into 3 groups: the 25% most successful, the 25% least successful, and the 50% falling between these extremes. The upper and lower quartiles were then compared on some 200 variables selected from the detailed ratings, test scores, and case-history data secured in 1922, 1928, and later. Some light is thus thrown on the factors that make for success or failure among subjects of fairly comparable intellectual endowment.

Control Experiments in Hypnosis and Their Relation to Certain Theories of Hypnosis. R. M. DORCUS, A. K. BRINTNALL, and H. W. CASE, University of California at Los Angeles.

The Relation Between Belief and Errors in ESP Recording. HOWARD F. UPHOFF, Stanford University. (Introduced by John L. Kennedy.)

Two comparable forms of an attitude scale to measure belief in telepathy were constructed using the Likert method. The scale yielded a reliability of .92 for the single forms and a reliability of .96 for the 2 forms when combined. When the scale was given to 2 groups, one known to be composed of believers, the other of disbelievers, no overlaps of scores were obtained.

This scale was used to select subjects (14 believers and 14 disbelievers) for a pseudotelepathic experiment in an attempt to measure the relation between belief and error commitment in recording 11,125 calls by usual ESP methods. Both groups made about an equal number of errors which had no effect upon the score; the believers tended to make errors that increased the score; the disbelievers tended to make errors that decreased the score.

Some Results From the Use of Special Tests for Automobile Drivers in the State of California. E. D. FLETCHER, Department of Motor Vehicles; and C. W. BROWN, University of California.

Special driving tests may be evaluated in terms of their capacity to differentiate good and poor drivers and in terms of the effect that their administration has on stimulating drivers to use their abilities to a greater advantage. The results obtained from administering a battery of special driving tests to many different groups of drivers in California indicate that some of the tests have high differentiating capacity. The use of the tests also has had a marked effect in reducing accidents in several of the groups.

Resemblance of Twins in Speed of Association. HAROLD D. CARTER, Institute of Child Welfare, University of California.

A free-association test was administered to 138 children ranging in age from 9 to 17 years. In the group were 36 pairs of identical and 33 pairs of fraternal twins.

Individual differences in speed of response were very reliably measured, as indicated by a reliability coefficient of .95. Coefficients of resemblance averaged .53 for identical twins, .44 for fraternal twins, and .00 for unrelated matched pairs. Treatment of different portions of the data showed that the greater resemblance of identical twins is slight but very consistent.

The results agree with other evidence concerning twin resemblances in personality traits. As compared with data on physique and intelligence, data on personality traits seem frequently to show lower coefficients of twin resemblance and a lesser difference between coefficients of resemblance for identical and for fraternal twins.

A Study of Commercial Automobile Drivers. A. K. BRINTNALL, University of California at Los Angeles. (Introduced by Knight Dunlap.)

The Imagery of Oriental Children. JOSEPH MORSH, University of British Columbia.

Overnight Changes in Pursuit Learning With 2 Distributions of Practice. M. **Brewster Smith**, Stanford University. (Introduced by E. R. Hilgard.)

Two groups of 22 undergraduates practiced the Koerth pursuit rotor on 4 consecutive daily sessions, each lasting 25 minutes. One group had 13 one-minute trials each day, separated by 1-minute rest periods, while the other had 8 one-minute trials separated by 3-minute rest periods, except for the first and last periods, which were 1 minute long.

Though the initial scores of the 2 groups were equal, the "spaced" group was markedly superior to the "massed" when comparison is based on mean scores after equal numbers of trials. This difference persisted throughout the trials on which comparisons were possible. The 2 groups were essentially equal throughout the 4 days in terms of mean scores after equal lengths of time from start of practice, although the "massed" group had practiced nearly twice as much. This appears to support the "stimulation-maturation" hypothesis. Overnight losses, which tended to increase from day to day, were minor and did not disturb the above relationships.

The Difference Limen for Tonal Duration. **Franklin Henry**, University of California.

The limens were determined by a modification of the constant method employing balanced series. Preliminary training reduced practice effects. The tones were produced electrically, the circuit being arranged to give a gradual build-up and termination of each sound in order to minimize transients. A motor-driven duration and increment controller was used.

The Weber ratio for a 50-db., 500-cycle tone was constant at 0.2 for durations of 47 to 277 milliseconds, but increased to nearly 0.3 at 32 milliseconds (average of 7 subjects). It was substantially unaltered by changing intensity from 30 to 90 db. above threshold, but increased at 20 db., particularly at the shortest duration. Changing frequency from 500 to 2000 cycles did not alter the Weber ratio, but it was increased approximately one-third at 250 cycles, a point of considerable theoretical interest.

The Relationship Between Basal Physiological Functions and Intelligence in Adolescents. **Nathan W. Shock** and **Harold E. Jones**, University of California.

A group of 43 girls and 44 boys from Oakland, California, was studied. Determinations of oxygen consumption, pulse rate, blood pressure, vital capacity, and respiratory volume were made under basal conditions on each of 2 successive mornings. In order to minimize the effects of training on the results of the physiological tests, observations collected in the Spring of 1935, when the children had a mean age of 14 years, were used for this study. This represented the seventh testing of these cases. The intelligence test used was the Terman Group Test. Forms A and B were administered at approximately 2-week intervals. Product moment correlation coefficients between mental test scores and the physiological variables were computed for males and females separately. The highest correlation obtained was +.27 between Terman Group Test scores and basal metabolism, expressed in calories per square meter per hour. None of the correlations were statistically significant.

One-Trial Learning in Rats. Bradford B. Hudson, University of California. (Introduced by C. W. Brown and Warner Brown.)

Fourteen rats, 24 hours hungry, were shocked once on the mouth by a condenser discharge as they ate from a metallic food holder attached to the center of a black and white striped cardboard pattern. They were tested without reinforcement at approximately monthly intervals for fear of this baited design.

On the first test trial, 1 month after the original experience, 13 gave marked negative responses. From this point on, the death rate was high among the animals due to old age, but at 6 months, 5 of the 7 still living showed definite avoidance responses. At 8 months, 2 of the 4 still living were " cautious " in their approaches to the bait, and 2 showed no fear.

One of the several sensitive indexes of a disturbed condition in the rat is provided by his sawdust-pushing behavior. His reaction on the first test trial is to cover, sometimes completely, the 3-inch-high design. The amount of sawdust pushed diminishes with each subsequent test trial.

A Method of Studying Olfactory Discrimination in the White Rat. Robert L. French, Stanford University. (Introduced by C. P. Stone.)

Small groups of animals were trained, in a specially designed multiple choice apparatus, to reach with their forepaws for unadulterated food in preference to food to which had been added quinine sulphate and 1 of 10 different odorous substances. Operative and environmental controls were introduced to rule out position, taste, visual and extraneous olfactory cues. The results demonstrate the dependence of the habit upon olfaction, great rapidity of learning, and the sensitivity of the rat to all of the odors used; and they suggest that olfaction is better developed in the rat and more important for its behavior than the results of earlier and more artificial techniques have implied.

Hesitation Time and Correctness of Choice in Rats. C. W. CRANNELL, University of California. (Introduced by E. C. Tolman.)

In a first experiment a simple elevated maze was used, and 12 rats were required on their second run to take the path in the opposite direction to the path taken on their first run. Mean hesitation time at the choice point over a period of 20 days was 9.5 seconds for correct, 7.2 seconds for incorrect, choices. The difference was significant.

In a second experiment 4 elevated pathways spreading fan-wise from a choice platform were used. Fourteen rats were required to take each path once only on any day, in any order they chose. Thirteen solved this problem. The results tended to show that the time spent on the choice platform was longest on the days just preceding solution.

The 2 experiments indicate the possible importance of hesitation at the choice point as a factor in the correctness of choice.

The Use of Discrimination Habits by Rats to Follow the True Pathways of Unfamiliar Maze Patterns. CALVIN P. STONE, Stanford University.

Blind rats were given preliminary training on simple elevated mazes with inclining pathways. The patterns of these simple mazes were changed each trial in the hope of teaching the rats to make discriminatory responses at each crossroad. This result was achieved in approximately 50 trials. Then the rats were confronted with new patterns that had from 12 to 18 choice points. Two trials per day were allowed, and a new pattern was introduced daily. All of the rats demonstrated remarkable ability to follow the true pathway of these complicated patterns. The best performer in a group of 7 made but a single error in 14 trials on 7 complicated patterns. As a general rule each animal made at least 1 perfect run on each new pattern. Choices at the junctures appeared to be based on proprioceptive cues afforded by the inclined treadways.

In a second phase of the study, elevated mazes with horizontal treadways were used. Half of the treadway of each unit was covered with fine wire screening. Two groups of rats were trained daily on simple patterns involving from 1 to 4 turns. For one group the true pathway utilized the screened surface; for the other, the smooth wood. On each trial the pattern was changed in order to set up a discrimination habit at the choice points. After approximately 40 to 60 trials, stable discriminations were established. Next, the rats were given trials on complex maze patterns that required from 10 to 20 choices. Again, the animals demonstrated remarkably good ability to follow the true pathway of new patterns, some of them making perfect runs on from 3 to 5 new patterns and none of them behaving as animals usually do in transfer situations.

The relevancy of these results to published data on maze studies will be discussed.

Application of the Cluster Analysis Technique to Profile Correlations of Rats With Brain Lesions. LLOYD V. SEARLE, University of California. (Introduced by C. W. Brown and R. C. Tryon.)

Scores were obtained in 30 measures of behavior for a total group of 45 rats divided into 4 subgroups, 1 of normals and 3 of operates differing with respect to locus of brain lesions. Behavior profiles, based on normalized standard scores in the 30 variables, were correlated, and the matrix of intercorrelations thus obtained was analyzed for "types" of behavior patterns by the Tryon method of correlation profile analysis. A complicated, but interesting, set of relations was found between clusters and loci of brain lesions.

Comparative Cluster Analysis. ROBERT C. TRYON, University of California.

A comparative treatment is presented of the operational unities discovered by cluster analysis in verbal and performance tests of children and adults, and in learning, motivation, and emotionality in rats. For children, the data analyzed are those of Schiller and of the Mooseheart Study (Holzinger). For adults, the data are those of Schneck, and of the Primary Trait Study (Thurstone). The animal materials analyzed are from the experiments of R. L. Thorndike on different measures of learning, of Anderson on different indices of motivation, and of Tryon on measures of learning and "personality." In each study the various test clusters and relations between them are presented, and tentative hypotheses of the radical psychological structure of these operational unities are formulated.

A Formalized Method for Constructing Objective Examinations. Jane Loevinger, University of California. (Introduced by R. C. Tryon.)

A method is presented for facilitating construction, on a large scale, of objective examinations. The method consists of having a standard list of relational forms usable for various subject matters. Some relations frequently used in multiple choice and true-false sentences are outlined, together with lists of synonyms and modifiers. Data are presented on the relative validity and difficulty of different forms.

Strong Vocational Interest Scores and Completion of Training in a School of Nursing. Josephine R. Hilgard, Stanford University.

The Strong Vocational Interest Test was administered to the entering students of St. Luke's School of Nursing in San Francisco, in order to determine the extent to which scores for nursing interest would predict both continuance in training and success in training. The tests were not scored until 2 years of training had elapsed, in order to avoid a spurious influence of suggestion by counselors. It was found that those with ratings on the test below "A" in nursing showed little likelihood of completing the nurses' training course. Of those with "A" ratings who did not continue, the largest proportion dropped out because of low grades. The interest scores of those remaining in service did not predict either grades in probationary courses or ratings of practical work in the wards. For these predictions, an intelligence test was more serviceable than the interest test. Low interest scores predicted chiefly those who would leave training in spite of their ability to do the intellectual work required.

The Social Meaning of Mood. Winifred B. Johnson, Stanford University.

Data will be presented to indicate that personalities subject to marked changeability of mood are as highly valued in the rôles of friend or associate as are personalities of equable emotional tone. This is (according to current findings) in contrast with the effect on marital adjustment and on personal happiness.

The social meaning of mood will be evaluated in terms of inter-friend judgments regarding certain other aspects of personality. In the thinking of friends and associates the moody personality is practically synonymous with the highstrung and tense personality. Intercorrelations suggest that (contrary to the verbal mores) the social value of " cheerful " attitudes is very slight.

The basic data for this minor contribution are the pooled rankings of 30 judges applied to each other as 2 groups of subjects (15 in each group). The persons involved had lived and worked closely together for periods of 1 to 4 years. Methodological findings indicate that social judgments so obtained have exceptional validity.

Color Vision in Relation to Artistic Ability. SUSAN ATWELL, Stanford University. (Introduced by P. R. Farnsworth.)

In several of his papers Alfred Adler has stated that inferiorities may result in overcompensation or even talent. As examples he has cited cases of colorblind painters.

In this study, 1213 third-, fourth-, fifth-, and sixth-grade students were given the color vision section of the Los Angeles Art Test, a group test in which the subjects match 48 test colors against 6 standard colors. The children were rated on general artistic ability by their teachers.

Chi squares and critical ratios were computed separately for each grade. In the fourth and fifth grades small but significant positive relationships were found between color scores and art ratings. The trend was in the same direction in the third and sixth grades, but did not reach significance.

These results seem to indicate that for the scholastic levels tested there is an effect which is opposed to what should be expected from Adler's hypothesis.

An Auditory Analysis of the Vibrato. RONALD MAKEPEACE, University of Southern California. (Introduced by Milton Metfessel.)

Recently there has been advanced an hypothesis that the frequency of modulation of the vibrato can be heard as a set of steady pitch components which correspond in position and relative intensity to those which are predicted by a Fourier analysis. Sets of phonograph records were constructed for 4 vibratos with differing rates, extents, and pitches. A set of steady tones covered the extent of each vibrato and made it possible to locate approximately the position of any components which could be heard.

Results indicate that there are salient pitches present, determined by the range and pitch of the vibrato, but that they do not correspond in position of relative intensity to those demanded by mathematical theory.

What Governs the High School Student in His Preference for Plays? Ralph H. Gundlach, University of Washington.

Forty plays were selected and rated by a group of dramatists who, for 2 years, had been producing plays before an extensive high school audience. From 3 to 8 of the 40 plays were read in each of 32 classes in high school English or drama, rated, and compared. From these reports it was possible to work out an approximate rank order for the students. No correspondence exists between the ratings by the dramatic experts and by the students. A basis for analyzing the plays was worked out and the determining factors approximately identified. These factors include the analysis of action, personalities, situations, the intellection, general mood, and the effective climaxes. The bases of the youngsters' selection involve familiarity with the personalities and problems of the characters, and a romantic sentimentality in treatment.

An Analysis of the Behavior of Critics. Paul R. Farnsworth, Stanford University.

It can be argued that consistently similar criticisms of art events must have been written by men of somewhat similar backgrounds. This paper describes an indirect method devised to probe the backgrounds of newspaper art and music critics. The plan was to learn the degree of resemblance between the reviews written by certain metropolitan critics. College subjects of less than average abilities in the arts were asked to read sets of 3 criticisms each and to judge how readily these could have been written by the same reviewer. For comparative purposes the subjects' responses were divided into 2 pools. These 2 groups of students agreed quite well both in their abilities and in their failures to select striking resemblances from among the criticisms. Thus, the experiment has demonstrated that relatively inartistic and unmusical subjects can rate criticisms with considerable consistency.

The Epicurean Theory of Imagination. Kate Gordon, University of California at Los Angeles.

Images of objects continually stream off and enter our minds and are bodily in texture. Novel shapes may be formed by combinations of such idols even before they enter our minds. Visions of the dead are merely such images and are not souls from Hades. When many images from similar objects come to us, combinations are formed which give us concepts or anticipations, so that we can interpret incoming stimuli. Many idols are in mind which we do not think of until the mind ' strains itself ' to be aware of them. This allows for freedom of choice. Yet the will requires an image through which to act. With the dispersal of the atoms of the body all mental life disappears.

Errors in Trial and Error Learning. WARNER BROWN, University of California.

The term ' trial and error ' describes a *form of behavior.* A trial and error *situation* is one in which some proportion of the movements are fruitless or perhaps lead to unpleasant effects. Trial and error *learning* occurs if the organism, in the course of successive encounters with such a situation, tends to reduce the proportion of fruitless movements, called " errors."

It has been questioned whether the errors contribute by their occurrence to the process of learning. The answer depends upon (a) the *number* of errors which it is possible to make and (b) the identifiability of the erroneous movements—if an error is identifiable there will be a tendency not to repeat it, but, if the erroneous movement is one of a large number of movements with no identifying characteristics other than being " not right," its influence in the learning process will be negligible.

An Experimental Study of Fixation of Response by College Students in a Multiple Choice Situation. ROBERT M. GOTTSDANKER, University of California. (Introduced by Warner Brown.)

Subjects were given a semisolvable problem on a multiple choice board. It was found that subjects working under experimental conditions which would make discrimination among choice possibilities poorer than that of the other subjects showed less variability of response. It is suggested that the fixation found by other observers is also understandable on the basis of lack of differentiation among choice possibilities.

Factorial Analysis of Selected Items From the Bernreuter Personality Inventory. G. M. KUZNETS, Stanford University.

Changes in Attitude Toward " Telepathy " and " Clairvoyance " During a 25-Year Period. JOHN L. KENNEDY, Stanford University.

In this paper cultural changes which have taken place with respect to the " feeling of being stared at," the reported frequency of premonitions and visions, and personal parapsychological experience are compared (1) between responses of students in psychology classes at Stanford during 1913–1917 and students in the University at the present time, and (2) between responses of students in 1913–1917 and the responses of a sample of 173 of these individuals contacted again in 1939. The direction of the changes in attitude is in the direction of less belief in telepathic and clairvoyant ability in the groups tested at the present time. Significant sex differences in favor of stronger belief in the women subjects were found in every comparison.

Conditioned Eyelid Responses as Mediating Generalized Conditioned Finger Reactions. ARTHUR A. LUMSDAINE, Stanford University. (Introduced by E. R. Hilgard.)

An "indirect conditioning" experiment, following essentially the procedure of a study reported by Shipley in 1933, was conducted as follows: In the first stage, a faint flash of light was repeatedly followed by a tap on the cheek which evoked a reflex eyelid response. In the second stage, the tap on the cheek was paired a number of times with an electric shock which evoked withdrawal of the finger. In the third stage, or test, the light was presented alone. Conditioned finger withdrawals occurred in an appreciable proportion of subjects in addition to conditioned eyelid responses, although the light had never before occurred in conjunction with the finger response. The finger response was absent if the lid response failed to appear. Photographic recording made detailed analysis possible. These results support the hypothesis that such "indirectly" conditioned finger responses may be mediated by the concomitants of the eyelid response which is a common feature of the 2 conditioning stages.

The Gambling "Instinct." FRANK C. DAVIS, University of California at Los Angeles.

It is suggested that the increased interest in gambling activities during recent years may be investigated as a phenomenon in the general field of human learning. Data collected from habitués of bookmaking establishments indicate the probability that we can ascertain under what conditions gambling arises and also what the effects of habitual gambling are in the individual. The data referred to consist of notes made immediately following interviews with patrons of bookmaking parlors, of notes and diagrams concerning observable aspects of their gross behavior, singly and in groups, prior to, during, and after the running of races, and of check-up data concerning the validity of the statements made during the interviews. The subjects came from a wide variety of occupational groups and were representative of a similarly wide variety of income levels. The following groups were distinguishable: (1) those who say they play the races every day because they are " fed up with their jobs "; (2) those who, whether or not they are bored with their work, have come to feel that in this present-day world it is sensible to live each day for whatever thrills the day may be made to yield; (3) those who make the vagaries of horse racing their hobby (but these persons, happy enough in their jobs, often follow the races, much of the time, merely on paper, without risking money, and in such individuals the psychological reaction is more intellectual, less emotional); (4) those few individuals who actually do make a living from their betting and have been so doing over a period of time. The persons in the first 2 groups admit that they are losing money, and many of them expect to keep on losing, but they feel that the thrills are worth it. The organic basis of these reported " thrills " is evident, especially when the individual makes a bet much larger than is his custom, in profuse perspiration, muscle tremors, and tic-like responses as the " crisis " reaches its height. Instrumental records of such responses are to be obtained in an extension of the investigation.

SYMPOSIUM

CHAIRMAN, ROBERT C. TRYON, UNIVERSITY OF CALIFORNIA

Psychological Research and Problems of Public Policy. E. R. HILGARD, Stanford University.

Relationship Between Information About the Labor Movement and Attitude Toward Labor Unions. FLOYD RUCH, University of Southern California.

The Value of Experimentally Creating Groups for the Study of Political and Social Issues. KURT LEWIN, State University of Iowa.

The Understanding of Social Issues Requires Participation in Their Solution. RALPH GUNDLACH, University of Washington.

The Importance of Conceptual Formulations in the Psychological Study of Social Issues. FRANKLIN FEARING, University of California at Los Angeles.

Experiments on Democratic and Autocratic Atmospheres. KURT LEWIN, State University of Iowa. (Slides and motion pictures.)

PROCEEDINGS OF THE TENTH SPRING MEETING OF THE EASTERN PSYCHOLOGICAL ASSOCIATION

H. S. OBERLY, SECRETARY-TREASURER, UNIVERSITY OF PENNSYLVANIA

The Tenth Annual Spring Meeting of the Eastern Psychological Association was held at Bryn Mawr College on March 31 and April 1, 1939. The meetings were attended by 660 individuals, 351 of whom were Members or Associates of the American Psychological Association.

The program included 113 scientific papers, 4 round table discussion groups, and a general session with 3 papers. The presidential address was delivered on Friday evening in Goodhart Hall, Bryn Mawr College, by Karl M. Dallenbach, Cornell University. The title of his paper was: " Pain: History and Present Status." Following this meeting a reception for members and guests was held by Bryn Mawr College, which opened its dormitories and dining halls for the use of those attending.

The American Psychological Association Committee on Displaced Foreign Psychologists sponsored a luncheon which was held on Friday, and the Psychologists' Committee for Aid to Spanish Democracy held a luncheon on Saturday.

Elections. President, 1939–1940: F. L. Wells, Harvard University; Secretary-Treasurer, 1939–1940 (to fill the unexpired term of H. S. Oberly, resigned): Harry Helson, Bryn Mawr College; Board of Directors: Theodora M. Abel, Trade Extension Classes, New York City, 1939–1942; George W. Hartmann, Teachers College, Columbia University, 1939–1942; H. S. Oberly, University of Pennsylvania, 1939–1940.

Committees Appointed. Program: E. Heidbreder, Wellesley College, Chairman; Ross A. McFarland, Harvard University; L. W. Max, New York University. Nominating: George Kreezer, Cornell University, Chairman; Myrtle B. McGraw, Columbia Medical Center; O. H. Mowrer, Yale University.

The Association approved the following recommendations of the Board of Directors: (1) that the future meetings of the Association will be 2-day sessions; (2) that a committee be appointed to study the question of membership as well as the operation of the Association as prescribed in the present by-laws.

The financial statement of the Association for the year 1938–1939 follows:

Income

Membership Dues	$452.00	
Guest Fees	79.76	
Interest on Savings Account	4.51	
Redemption of Envelopes	5.02	
From 3 Organizations for Printing and Mailing Luncheon Notices	9.00	
		$550.29

Expenses

Printing of the Proceedings for 1938 in the *Bulletin*	$23.21	
Postage, Telephone, Telegrams	70.30	
Printing Programs, Mimeographing, and Preparation for Mailing	138.41	
Corrections to Mailing List	16.50	
Program Committee Expenses	24.18	
Secretary-Treasurer Office	78.34	
Contribution to A. P. A. Committee on Displaced Foreign Psychologists	50.00	
		400.94
Profit for the year 1938–1939		$149.35

Balance Sheet

Cash:	Fifth Avenue Bank	$524.62	
	New York Savings Bank	304.83	
			$829.45
Capital:	As of June 1, 1938	$680.10	
	Profit, 1938–1939	149.35	
			829.45

Next Meeting: The Eleventh Annual Meeting of the Eastern Psychological Association will be held in Atlantic City, New Jersey, on Friday and Saturday, April 5 and 6, 1940.

The following papers were presented:

EDUCATIONAL PSYCHOLOGY

CHAIRMAN, G. W. HARTMANN, COLUMBIA UNIVERSITY

The Adequacy of Scales of Developmental and Emotional Age, When Applied to a Group of Deviates. ROBERT L. THORNDIKE, Columbia University.

Learning Ability as a Function of Age. DONALD E. BAIER, New Jersey State Hospital.

The Measurement of Study Habits. ARTHUR C. HOFFMAN, University of Rochester.

A Comparison of the Performance of Freshmen and Sophomores in a Beginning Psychology Course. M. BRUCE FISHER, Rhode Island State College.

Study of the Reliability and Validity of the Academic Reference Blank. VINCENT J. SHARKEY and JACK W. DUNLAP, University of Rochester.

The Science-centered vs. *the Student-centered General Psychology Text as a Preparation for Educational Psychology.* EDWARD B. VAN ORMER, Pennsylvania State College.

Methods by Which Female College Students Select Vocations. ROBERT TYSON, Hunter College.

ATTITUDES

CHAIRMAN, HADLEY CANTRIL, PRINCETON UNIVERSITY

An Experimental Study of Social Vectors (Prejudice) in Reasoning. SAUL B. SELLS, U. S. Works Progress Administration.

An Analysis of the Operation of Authoritative Standards in Political Judgment. HELEN BLOCK LEWIS, Brooklyn College.

Changes in the Attitudes of Student Political Groups Towards Methods of Preventing War as a Result of the Czechoslovakian Crisis and the Peace of Munich. BERNARD F. RIESS, Hunter College.

An Analysis of Attitudes Toward Fascism and Communism. DANIEL KATZ and HADLEY CANTRIL, Princeton University.

The Isolation and Measurement of General Attitudes. LEONARD W. FERGUSON, University of Connecticut.

Attitudes of College Students and of Foremen in Industry on Personnel Policies. BRUCE V. MOORE, Pennsylvania State College.

SENSORY PSYCHOLOGY

CHAIRMAN, WOLFGANG KÖHLER, SWARTHMORE COLLEGE

A Substitute for "Cold Spots." WILLIAM L. JENKINS, Lehigh University.

Preliminary Report on the Differential Spectral Hues. D. FARNSWORTH, New York University. (Introduced by Douglas Fryer.)

On the Neurophysiology of Visual Intensity Discrimination. A. H. HOLWAY and L. M. HURVICH, Harvard University.

The Estimation of Relative Saturation. SIDNEY M. NEWHALL, Johns Hopkins University.

The Theta Effect: An Experimental Demonstration of Certain Principles in Perceptual Orientation. FREDERICK C. THORNE, Hunter College.

Further Studies in Inter-Sensory Gestalten. G. M. GILBERT, Columbia University.

Some Variations in the Size-Weight Illusion (Preliminary Report). K. W. OBERLIN, University of Delaware.

APPLIED PSYCHOLOGY

CHAIRMAN, D. A. FRYER, NEW YORK UNIVERSITY

Man-caused Forest Fires as a Problem in Applied Psychology. JOHN P. SHEA, U. S. Department of Agriculture.

Employee-Choice of Phonographic Music To Be Played During the Work Spell. JOHN F. HUMES, Pennsylvania State College.

The Rules of Evidence—An Empirical Study in Legal Psychology. STEUART H. BRITT, George Washington University.

Some Factors Which Affect the Outcome of an Interview. S. M. HARVEY, Smith College.

Differential Scores of Speaking Ability: Norms for 3 Revised Forms of the Bryan-Wilke Scale. A. L. BRYAN and W. H. WILKE, Pratt Institute and New York University.

Relationship of Psychometric Scores to Parole Success of High-grade Mentally Defective Girls. ROY HAMLIN, Letchworth Village.

BRAIN FUNCTION

CHAIRMAN, K. S. LASHLEY, HARVARD UNIVERSITY

Vestibular Dysfunction Following Lesions of the Eighth Nerve. W. D. NEFF, University of Rochester (introduced by K. U. Smith); and K. U. SMITH, University of Rochester.

Pupillary Function in the Cat Following Lesions to the Striate Cortex. WILLIAM E. KAPPAUF, University of Rochester.

The Effect of Frontal Lobectomy Upon Weight Discrimination in the Monkey. T. L. McCulloch, Yale University.

The Effect of Removal of Both Frontal Association Areas of the Human Brain (bi-frontal "lobectomy"): Report of a Case Without Mental Signs. D. O. Hebb, McGill University.

The Behavior of Decorticate Guinea Pigs. Karl U. Smith, University of Rochester.

Electroencephalographic Findings in Behavior Problem. W. E. Rahm, Jr., Columbia University.

The Comparative Development of the "Occipital" and "Pre-Central" Alpha Rhythms. J. Roy Smith, Columbia University.

Experimentally Induced Conflict

Chairman, H. S. Liddell, Cornell University

Spatial Gradients of Approach and Avoidance Responses as Measured by a Work Technique. Judson S. Brown, Yale University.

An Experimental Analysis of Approach-Approach and Avoidance-Avoidance Conflict. Seymour G. Klebanoff, Yale University.

The Influence of Place of Punishment Upon Behavior in Approach-Avoidance Conflict Situations. Neal E. Miller, Yale University.

Behavior Disturbances in the Rat Resulting From Prolonged Training Upon Antagonistic Spatial Habits. H. A. Witkin, New York University.

"Anxiety States" in Cats Following Experimental Convulsions. James D. Page, University of Rochester.

Abnormal Psychology

Chairman, R. R. Sears, Yale University

Respiratory "Arrests" During Day Dreaming in Normals and in Schizophrenics. H. Barry, Jr., Tufts College.

Time Estimation in Schizophrenics. J. McVicker Hunt and Harriet M. Johnston, Brown University.

Set in the Schizophrenic as Determined by Length of Preparatory Interval in Reaction Time. E. H. Rodnick and D. Shakow, Worcester State Hospital.

Endocrinological Treatment of Homosexuality. L. W. MAX, New York University.

A Study of Mental Deterioration in Ideopathic Epilepsy by an Analysis of Word Meaning and Usage. HARRY M. CAPPS, Columbia University. (Introduced by Carney Landis.)

Measurement of Spontaneous Movements in Psychotic Adults by the Time-Sample Technique. MARSHALL R. JONES, Yale University.

Stuttering in Relation to Position in the Family. JULIAN B. ROTTER, Worcester State Hospital. (Introduced by David Shakow.)

ANIMAL MOTIVATION

CHAIRMAN, C. J. WARDEN, COLUMBIA UNIVERSITY

A Study of Food Behavior by the Method of Paired Comparison. W. A. BOUSFIELD, University of Connecticut.

The Effects of Competition Upon Goal-seeking Behavior in the Rat. CHARLES N. WINSLOW, Brooklyn College.

Latent Learning and the Goal Gradient Hypothesis. CLAUDE E. BUXTON, Swarthmore College.

Intensity of Drive and the Behavioral Distance to a Goal. RICHARD S. CRUTCHFIELD, Swarthmore College.

The Waning of Maternal Drive in the Guinea Pig. JOHN P. SEWARD, Connecticut College.

Is the "Sex Drive" in the Male Guinea Pig Specific or General? GEORGENE H. SEWARD, Connecticut College.

Instrumentation in Monkeys on a Multiple Platform Setup. W. GALT and C. J. WARDEN, Columbia University.

Research Film: *The Behavior Pattern of Central American Army Ants.* T. C. SCHNEIRLA, Washington Square College.

GENERAL AND EXPERIMENTAL

CHAIRMAN, S. W. FERNBERGER, UNIVERSITY OF PENNSYLVANIA

Operations in Psychology. FORREST L. DIMMICK, Hobart College.

The Association of Color and Musical Mood. HENRY S. ODBERT, THEODORE F. KARWOSKI, and A. B. ECKERSON, Dartmouth College.

Musical Preferences in Relation to Social Personality. JUNE

CARPENTER, Connecticut College. (Introduced by John P. Seward.)

Differences in Responses to Good and Poor Poetry. CATHARINE PATRICK, Columbia University.

A Quantitative Study of the Development and Decay of an Experimentally Induced Expectancy. O. H. MOWRER, Yale University.

A Modification of the Ach Test of Conceptual Thinking. J. Q. HOLSOPPLE, New Jersey State Hospital.

Authoritarianism in Science and Extra-Sensory Perception. H. ROGOSIN, New York University.

VISION AND AUDITION

CHAIRMAN, C. W. BRAY, PRINCETON UNIVERSITY

Visual Brightness Discrimination in the Cat as a Function of Illumination. LEONARD C. MEAD, University of Rochester.

Discrimination of Spectral Colors by Chimpanzee. W. F. GRETHER, Yale University.

Alterations in Dark Adaptation Under Reduced Tensions of Oxygen. R. A. MCFARLAND and J. N. EVANS, Harvard University.

Hearing in the Guinea Pig Fetus. LEONARD CARMICHAEL, Tufts College.

The Mechanism of Hearing by Electrical Stimulation. S. S. STEVENS and R. CLARK JONES, Harvard University.

The Loudness-Intensity Function in a Region of Impaired Sensitivity. E. H. KEMP and JANE FLANNERY, Brown University.

TESTS AND MEASUREMENTS

CHAIRMAN, R. T. ROCK, JR., FORDHAM UNIVERSITY

Experimental Evidence on the Questionnaire as a Research Instrument. JOHN G. JENKINS, University of Maryland.

A Statistical Method for Determining and Depicting Intra-group Changes of Measure. WILLIAM D. TURNER, Institute of the Pennsylvania Hospital.

All or None vs. *Graded Response Questionnaires.* EDWIN E. GHISELLI, University of Maryland.

General Considerations in the Selection of Test Items and a Short Method of Estimating the Product Moment Correlation Coeffi-

cient From Data at the Tails of the Distribution. JOHN C. FLANAGAN, American Council on Education.

Teleonomic Description in the Study of Behavior. RAY S. MUSGRAVE, Russell Sage College.

Lateral Dominance in Feet. L. PEARL GARDNER, Cornell University.

GENETIC PSYCHOLOGY

CHAIRMAN, B. S. BURKS, CARNEGIE INSTITUTE

An Intelligence Test for Infants and Young Children. PSYCHE CATTELL, Harvard University.

A Genetic Study of the Naming of Visual Figures. FRANCIS W. IRWIN, University of Pennsylvania.

Three Aspects of the Genetic Development of Abstract Thinking. LIVINGSTON WELCH, Hunter College.

An Application of the Skinner Technique in the Study of Child Behavior. FRED S. KELLER, Columbia University.

Quantitative Measures in Studying the Development of Behavior Patterns Ascending Slides. A. P. WEINBACH, New York City. (Introduced by Myrtle B. McGraw.)

An Improved Discrimination Technique for Use With Young Children. LOUIS LONG, Columbia Medical Center.

PHYSIOLOGICAL PSYCHOLOGY

CHAIRMAN, A. T. POFFENBERGER, COLUMBIA UNIVERSITY

The Effect of Hypophysectomy on the Behavior of Dogs of Different Constitutional Types. W. T. JAMES, Cornell Medical College.

Glandular Dominance in Human Subjects. B. KORCHIN and A. L. WINSOR, Cornell University.

Preliminary Studies of Human Secretory Reactions During Conditions of Tension and Release From Tension. A. L. WINSOR and B. KORCHIN, Cornell University.

Experimental Observations on Changes in Skin Temperature Associated With Induced Emotional States. BELA MITTELMANN and HAROLD G. WOLFF, New York City.

The Effect of 2 Grains of Caffeine (Alkaloid) on the Rate of Adding, Report of Boredom and Other Factors. JOSEPH E. BARMACK, College of the City of New York.

The Effect of Morphine Addiction and Withdrawal on the Sexual Functions of Chimpanzees. S. D. S. SPRAGG, Barnard College.

Psychological Correlates of the Electroencephalogram. A. C. WILLIAMS, JR., New York State Psychiatric Institute and Hospital.

HUMAN LEARNING

CHAIRMAN, J. W. DUNLAP, UNIVERSITY OF ROCHESTER

Conditioned-Response Learning as a Function of the Length of the Temporal Interval Between Paired Stimulations. JAMES S. CALVIN, Yale University.

Changes Occurring in the Serial Reproduction of Verbally Perceived Materials. MARGARET E. TRESSELT, Columbia University. (Introduced by S. D. S. Spragg.)

Intra-List Generalization as a Factor in Verbal Learning. ELEANOR J. GIBSON, Smith College.

Evidence of Organization in the Learning of Nonsense Syllables. EDWIN B. NEWMAN, Swarthmore College.

Transfer of Training and Application of Principles. GEORGE KATONA, New School for Social Research.

A New Type of Mental Growth Curve. T. W. RICHARDS, Antioch College.

CONDITIONED RESPONSE

CHAIRMAN, W. S. HUNTER, BROWN UNIVERSITY

Reflexive and Attitudinal—or Quantitative and Qualitative—Conditioning. G. H. S. RAZRAN, Columbia University.

Generalization of Conditioned Psychogalvanic Responses Following 2 Conditions of Reinforcement. LLOYD G. HUMPHREYS, Yale University.

Indirect Conditioning of Forelimb Flexion in Dogs. W. J. BROGDEN, Johns Hopkins University.

Differential Effects of Curare in the Central Nervous System. E. K. CULLER, University of Rochester.

Further Observations on "Conditioning" and "Extinction" in the Spinal Dog. P. S. SHURRAGER, University of Rochester.

An Exploratory Study on the Effect of Oxygen Deprivation on Periodic Reconditioning and Extinction in Rats. CLIFFORD P. SEITZ and FRED S. KELLER, Columbia University.

INTELLIGENCE

CHAIRMAN, L. N. YEPSEN, NEW JERSEY DEPARTMENT OF INSTITUTIONS AND AGENCIES

The Interpretation of IQ's on the 1937 Revision of the Stanford-Binet Scales. FREDERICK B. DAVIS, Avon Old Farms.

A Comparative Study of the 1916 Stanford-Binet Intelligence Test and the 1937 Stanford-Binet Intelligence Test (Form L) on the College Level. BERNARD SLESS, Temple University. (Introduced by Hughbert C. Hamilton.)

An Investigation Into Response Patterns on Revised Stanford-Binet (Form L) of Adult Male Delinquents With Mental Age Less Than 12. SEYMOUR M. BLUMENTHAL, Elmira Reformatory.

The Performance of Normal and Subnormal Adolescents and Children on a Simple Drawing Task. THEODORA M. ABEL, New York City; and JANE B. SILL, Yale University.

Variability in Nursery School. GRACE E. BIRD, Rhode Island College of Education.

PERSONALITY

CHAIRMAN, R. G. BERNREUTER, PENNSYLVANIA STATE COLLEGE

Personality Test Scores of Allergic and Nonallergic Children. OLGA DECILLIS and BERNARD F. RIESS, Hunter College.

Personal Attitudes of Maladjusted Boys. LAWRENCE RUBENSTEIN, Hebrew Sheltering Guardian Society.

A Correlation Study of Parental Influences Upon Adolescent Personalities. ROSS STAGNER, University of Akron.

A Personality Schedule for Adolescents. BEATRICE CANDEE, Vocational Service for Juniors, New York City.

The Validity of the Bell Adjustment Inventory for College Women. RUTH A. PEDERSEN, University of Rochester.

A Preliminary Report of a Psychological Study of the Northern Blackfoot Indians. A. H. MASLOW, Brooklyn College.

RORSCHACH TESTS

CHAIRMAN, F. H. LUND, TEMPLE UNIVERSITY

An Experiment to Test the Validity of the Rorschach Test. R. NEVITT SANFORD, MARGARET M. ADKINS, and ELIZABETH A. COBB, Harvard University.

Personality Diagnosis in Early Childhood: The Application of the Rorschach Method at the Preschool Level. BRUNO KLOPFER, New York City.

Diagnosis and Prognostic Value of the Rorschach Test in Neurological Cases. M. R. HARROWER, McGill University.

Personality Changes in Insulin-treated Schizophrenics. ZYGMUNT PIOTROWSKI, New York City.

Motivation and Stimulus-Structuration in the Process of Suggestion. THOMAS E. COFFIN, Princeton University.

ROUND TABLES

Functions of the School Psychologist. Chairman, T. E. NEWLAND, Pennsylvania Department of Public Instruction.

The Law of Effect and the Laws of Conditioning. Chairman, W. S. HUNTER, Brown University.

Critical Problems in the Formation and Change of Attitudes. Chairman, DANIEL KATZ, Princeton University.

Mechanisms in Visual Perception-Interrelations of Constancy, Contrast, Conversion, and Adaptation. Chairman, HARRY HELSON, Bryn Mawr College.

GENERAL SESSION

CHAIRMAN, KARL M. DALLENBACH, CORNELL UNIVERSITY

Recent Advances in the Conditioned Response. D. G. MARQUIS, Yale University.

Field Work in Social Psychology—The Study of Unemployment in a Scottish Industrial City. O. A. OESER, University of St. Andrews, Scotland.

Some Problems in the Measurement of Public Opinion. GEORGE H. GALLUP, American Institute of Public Opinion, New York City.

PROCEEDINGS OF THE EIGHTH ANNUAL MEETING OF THE ROCKY MOUNTAIN BRANCH OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

ROBERT H. BRUCE, SECRETARY, UNIVERSITY OF WYOMING

The Eighth Annual Meeting of the Rocky Mountain Branch of the American Psychological Association was held at the Colorado State College of Education, Greeley, Colorado, on Friday and Saturday, November 25 and 26, 1938.

At a brief business meeting, Dr. George T. Avery, of the Colorado State College, Fort Collins, Colorado, was elected Executive Secretary of the Rocky Mountain Branch for the coming 3 years.

PROGRAM

A Study of Personality Factors. W. A. BLAKELY and PHILIP J. THOMPSON, Colorado College.

Twinning as a Factor Influencing Personality. LILLIAN PORTENIER, University of Wyoming.

Student Employment and Student Class Load. J. D. HEILMAN, Colorado State College of Education.

Personal vs. *Form Letters for Contacting Prospective University Students.* W. P. REED, University of Wyoming.

A Qualitative Study of Types of Social Dominance Exhibited by Albino Rats in the Field Situation. LESTER H. GLIEDMAN and ROBERT H. BRUCE, University of Wyoming.

A Preliminary Investigation of Persistent Non-adjustive Behavior. I. KRECHEVSKY, University of Colorado.

Operationism as Method in Psychology. PAUL CRISSMAN, University of Wyoming.

An Example of the Psychology of Prestige. T. H. CUTLER, University of Denver.

A Summary of Race Psychology. THOMAS R. GARTH, University of Denver.

An Emotional Stability Quotient. V. E. FISHER, State Hospital South, Blackfoot, Idaho.

Some Aspects of the Psychological Care of the Tuberculous. MORTON A. SEIDENFELD, National Jewish Hospital, Denver, Colorado.

Mental Abnormality in Relation to Crime. PAUL DRAPER, Colorado Springs.

The Study of Radio Advertising. RICHARD J. TRIPLETT, University of Denver.

This meeting was held in connection with the Colorado-Wyoming Academy of Science, and the abstracts of the foregoing papers are published in the *Journal of the Colorado-Wyoming Academy of Science,* Vol. 2, No. 5.

THE REQUIREMENTS OF AN ADEQUATE ATTITUDE SCALE

BY LEONARD W. FERGUSON

University of Connecticut

An attitude may be defined as the *acceptance value of a belief.*[1] A belief is that which is expressed in a proposition as the outcome of inquiry (**10**, p. 7). This implies that by reference to the ongoing process of inquiry the belief can be judged true or false (**29**, p. 268). The acceptance value of the belief may be quite independent of its truth or falsity. To illustrate, consider the belief expressed in these words: "ESP cannot be explained by the known natural laws of sense perception." Some persons, as is well known, are wholly convinced of the belief expressed by this proposition, while others are quite convinced of the contrary. It is obvious that the statement cannot have 2 truth values, *i.e.* truth and falsity. It is now, or will become, one or the other of these upon continued inquiry into the pertinent phenomena. It is possible, however, for different people to have different acceptance values of the belief ranging from complete acceptance through neutrality to complete rejection. It is this series of acceptance values that constitutes the attitude continuum. The reader will note that this definition makes no assertion as to resultant behavior. Whether or not a person acts in accordance with his attitude is a question quite apart from a definition or measurement of it.

Letting the above definition serve as a brief introduction to the writer's point of view concerning the nature of attitude, he proposes to present a critique of current attitude-measurement methodology. His procedure will be to consider, in turn, each of 7 major requirements for an adequate attitude scale.

(1) The first requirement of a measuring instrument or scaling device is that it shall give results corresponding to an underlying physical order (**41**, p. 44). Since it is impossible to get any closer to the underlying physical order of an attitude than its expression, it is impossible to test this hypothesis directly in the field of attitude

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[1] Suggested by Professor E. W. Hall, Department of Philosophy, Stanford University.

measurement. This can be done, however: There can be adapted for the measurement of attitudes a method which, in other fields of scientific endeavor, has been shown to meet this requirement. This method, then, will be what Dewey, in his recent book, *Logic,* calls a *vera causa* (10, p. 3), *i.e.* a method which, in some field of inquiry, has been found to give verifiable results and therefore can be considered adequately grounded in the sphere of scientific inquiry. The method which Thurstone and Chave (43) chose as a *vera causa* in attitude measurement was that of *equal-appearing-intervals.* This method, originally developed by Delboeuf for the measurement of sensory discrimination (5), has been found to give results corresponding reasonably well to an underlying physical order (44). It is true that Ament (2) and Hevner (17) have pointed out that stronger or higher stimuli are underestimated in comparison with weaker or lower stimuli, and that Thorndike (40) and Hillegas (18) have found the results to disagree slightly with those obtained by other psychophysical methods. This criticism invalidates the use of the *equal-appearing-interval* method in strict measurement (8), but in the present instance the objection is not particularly cogent because in attitude measurement the method is used only as a scaling device (8). Work done by the writer (12) shows that scale values of statements computed by both the *equal-appearing-interval* and *paired-comparisons* methods correlate +.95 and over, and shows also, contrary to the findings of Hevner (17), that, as far as attitude scales are concerned, the relationship is linear. The only other method of attitude-scale construction that can claim to have as good a basis for being designated a *vera causa* as the method of *equal-appearing-intervals* is the *paired-comparisons* method, but certain limitations, pointed out later, make its unrestricted use in attitude measurement somewhat questionable.

(2) A second requirement of an adequate attitude scale is that scale values of statements chosen as landmarks not be affected by other items in the scale. In the *paired-comparisons* and *order-of-merit* methods the scale value of each item is determined by comparisons with other items in the scale and, thus, is a function of the particular ones included. Furthermore, as pointed out by Guilford (15), when either the *paired-comparisons* method or method of *similar reactions* is used as a single criterion, items having a scale difference of an irrelevant nature may remain undetected and would be given as much weight in determining a person's attitude as 2 items having a relevant scale difference. Some other method must be employed to eliminate irrelevant items from the scale. In the method

of *equal-appearing-intervals* the scale value of each item is determined in such a manner that it is entirely independent of values assigned to other items in the scale. One can substitute items having similar scale values for one another without affecting in any way the scale values of the other items. Likert's (**26**) and Rundquist and Sletto's (**33**) methods of attitude-scale construction are as satisfactory in this respect as the *equal-appearing-interval* method, but disadvantages mentioned below render these methods less valid than the latter.

(3) A third requirement of an adequate attitude scale is that attitudes of judges who sort the statements (when scale values are determined by a sorting procedure) or attitudes of persons taking a test (when scale values are based on their responses) not affect, markedly, scale values of the statements. Hinckley, working with scales for the measurement of attitude toward Negroes (**19**), Beyle, working with scales for the measurement of attitudes toward Alfred E. Smith during the 1928 Presidential Election Campaign (**3**), Ferguson, working with scales for the measurement of attitude toward war (**11**), and Pintner and Forlano, using scales for the measurement of attitude toward patriotism (**30**), have proved conclusively that scale values of items in a test constructed by the method of *equal-appearing-intervals* are not affected by the attitudes of the judges. All reported correlations between the scale values of statements, based on groups of judges differing significantly in their attitudes toward the issues involved, are +.97 or over. This fact has not been demonstrated to be true of scales constructed by methods other than that of *equal-appearing-intervals.* In fact, Sletto (**34**), using the criterion of internal consistency, has reported that scale values of statements based upon the response of different, but supposedly comparable, criterion groups correlated on the average only +.34 with each other. The highest correlation he reports is +.74. Furthermore, to stabilize the values he found that at least 400 persons had to be included in the criterion groups—a point peculiarly in line with Dr. Strong's experience in determining, by a criterion of internal consistency, weights for the items on the Vocational Interest Test.[2] On the other hand, results secured by Leuenberger (**25**), Uhrbrock (**45**), and myself (**11, 12**), using the method of *equal-appearing-intervals,* show that scale values based upon the responses of 50, or even as few as 25, persons correlate near unity with those

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[2] Personal communication to the writer.

based on the responses of three or four hundred persons. Consequently, both because of consistency of results and because of the fact that fewer persons are required to give that consistency, the *equal-appearing-interval* method, with respect to this particular point, is vastly superior to other methods.

(4) A fourth requirement of an adequate attitude scale is that it should be quite specific in content. The scales which Thurstone and his associates have constructed seem to meet this requirement better than other types of scales. For example, intercorrelations between various Thurstone scales are, as has been found by T. G. Thurstone (**42**), Carlson (**7**), and myself (**14**), rather low, ranging from .00 to approximately ±.35. This fact, however, is not an advantage inherent in the method of *equal-appearing-intervals,* for scales constructed by the Likert and Sletto techniques could probably be made just as specific in content as those of Thurstone. There are some methods, though, which do actually exclude such specificity. For example, the generalized attitude scales constructed by Remmers and associates (**31**) are of this type, for the statements in any one scale must be applicable to all of any selected class of attitudinal objects. Another type of scale which excludes specificity is the generalized type constructed by Kirkpatrick (**21, 22, 23**). This results, however, from his philosophy of attitude measurement rather than from the methodology employed. The reader, no doubt, has noted that the writer has used the word *general* as applied to attitude scales in 2 different senses.

Remmers means by a general attitude scale one that can be applied to any one of a selected class of objects, such as school subjects. The scale may be applied to the measurement of attitude toward any one subject by inserting in the appropriate space the name of the particular subject toward which the attitude is to be measured. The statements in the scale remain the same and have the same values regardless of the subject chosen. Stagner and Drought (**38**) have found, however, that statements in such a scale should not have the same scale values when applied to different members of the same attitudinal class—a fact which invalidates this type of scale.

The other type of general attitude scale is exemplified by that constructed by Kirkpatrick for the measurement of attitude toward feminism (**21, 22, 23**). Kirkpatrick states that he believes attitudes are general, not specific, and therefore selects, on *a priori* grounds, items which he thinks will be diagnostic of this generality. It may be that some attitudes are general in content; in fact, the writer agrees with Cantril (**6**) and G. W. Allport (**1**) that such is the case; but he also agrees with Stagner (36, 37) that attitude scales should consist of items quite specific in content. If there is any generality in attitudes it will be possible to extract it from the responses to the specific items or scales. That such is not an impossibility has already been demonstrated by T. G. Thurstone (42) and Carlson (7) in their factor analyses of various attitude scales. Corroborative and more complete evidence on this point by the writer appears elsewhere (14). The point is that the issue should not be prejudged by constructing only generalized attitude scales. The writer does not mean to imply that general attitude scales should not be constructed, for he is preparing for publication 2 such scales. However, the rationale underlying their construction is quite different from that which he is here criticizing. Furthermore, they will meet all of the requirements here laid down for an adequate attitude scale.

(5) Validity seems too obvious a requirement to mention. It is a surprising fact, however, that very few researchers have paid this point much attention. The scales constructed by Thurstone and associates, as one begins to expect, here again top the list. They are validated in the course of construction in that only those items which a sufficient number of persons allocate to the same scale position are chosen for retention in the scale. Secondly, the items meet a criterion of internal consistency—in this case that known as the criterion of irrelevancy. Only those items are retained which, if their scale values are similar, also have a similar number of indorsements. The third way in which these scales are validated is by giving them to criterion groups. Although data are meager on this point, those which have been published indicate that Thurstone's scales differentiate criterion groups fairly well. For example, Stouffer (39) finds a validity coefficient of .81 for the Smith scale for the measurement of attitude toward prohibition when scores on it are compared to ratings based upon the case-history method. Here it may be remarked that attempts similar to that which LaPiere has made to prove the invalidity of attitude scales (24) overlook the very important fact that actual behavior is not an adequate criterion upon which to base the validity of an attitude scale. In spite of that fact, however, Rosander (32) has secured results showing much greater agreement between behavior situations and verbal expression than LaPiere indicates would be probable.

Experimenters other than Thurstone or his students have validated their scales only by some criterion of internal consistency, the inadequacy of which as a single criterion has already been indicated, or have used Thurstone scales as a criterion. This latter method was used by Likert in the development of his technique for the measurement of attitudes (**26**), but further comment appears upon this point below.

(6) Reliability, as a requirement for an adequate attitude scale, is as obvious as that of validity, but some comment seems pertinent. Thurstone reports that reliabilities of scales constructed under his editorship are all over .8, most of them being over .9.[3] Likert, Roslow, and Murphy (**27**) report reliabilities for forms of 20 items of the Thurstone scales ranging from .42 to .84, and from .59 to .91 for forms of 40 items; Nystrom (**28**) reports a reliability of .92; Bolton (**4**) reports figures ranging from .30 to .46; and Stouffer (**39**) reports .94. The writer has secured reliabilities ranging frm .52 to .80 for the 20-item forms and from .68 to .89 for the 40-item forms. Thus, it is questionable, upon the basis of this evidence, whether the present Thurstone scales are better or poorer than scales constructed by other methods. Since there are 2 forms of every Thurstone scale, however, it is possible to make an additional check which cannot always be applied to other scales. The comparability of 2 forms of an attitude scale may be demonstrated by showing that both lie in the same factor space when the intercorrelations among a number of them are factor analyzed. The writer has already presented evidence that such is the case (**14**). The largest discrepancy he has found between any one correlation and the average of the set of 4 showing the same relationship for the 45 possible combinations of 10 of the Thurstone attitude scales (*e.g.* War A *vs.* Communism A; War A *vs.* Communism B; War B *vs.* Communism A; and War B *vs.* Communism B) is .12. Most discrepancies were much smaller.

(7) The seventh and last requirement which will be mentioned is that a scale should be a measure of a linear continuum. Thurstone and Chave (**43**) at one time suggested that the criteria of ambiguity and irrelevance were sufficient to accomplish the selection of statements on a linear continuum, but the writer has shown that such is not the case. In a factor analysis of Peterson's War Scale (**13**), the writer discovered that 3 factors were necessary to explain the intercorrelations among the items. If the criteria of ambiguity and irrelevance used in the construction of this scale had been sufficient to insure the selection of statements on a linear continuum, a factor analysis should have revealed only 1 factor. Sletto (**34**) states, without giving supporting evidence, that his " scale-value-difference-ratio " will pick out statements on a linear continuum. From Sletto's

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[3] Personal communication to the writer.

description of that criterion, however, the writer fails to see how such selection takes place; and, indeed, the findings of Smart (**35**) and Humphreys (**20**) that there is only 1 general factor running through all 6 scales that Rundquist and Sletto constructed would seem to indicate that the criterion did not operate to select statements upon a continuum.

Before concluding this article, the writer would like to call to the reader's attention a rather significant point in connection with Likert's technique of attitude-scale construction (**26**). He has reported that scoring scales by assigning values to different responses of the groups taking the test yields results somewhat more reliable than those secured by the Thurstone method of scoring. He concluded from this fact that a judging group was unnecessary. To establish this point he gave to a group of subjects the Droba War Scale (one constructed by the *equal-appearing-interval* method), asking them to indorse or reject each statement in accordance with the usual Thurstone directions. He also gave to this group this same list of statements (less 2), with 5 possible responses listed in front of each one. These were the 5 usual ones, ranging from complete acceptance of the statement through neutrality to complete rejection. He found, as one might quite obviously expect, a high correlation between these 2 methods of scoring practically the same set of statements. He found, further, that his own method gave the more reliable results. This is, of course, what one would expect, as it has been repeatedly demonstrated (**9**) that as the number of possible responses increases (up to certain limits) reliability is also increased. Likert concluded from these data that his method of scoring did away with the need for a judging group. A scale which Likert constructed independently of Thurstone's method yielded correlations of only .65 and .71 with the latter; hence the writer believes Likert's conclusion concerning the judging group unsubstantiated.

It is now obvious to the reader that the attitude scales constructed by the method of *equal-appearing-intervals* satisfy more of the requirements than do those constructed by any other method. The two which it doesn't, at present, meet wholly satisfactorily can easily be rectified. The linearity of the scales can be insured by factor analyzing the item intercorrelations and eliminating the irrelevant items; the reliabilities will also be increased by this operation and undoubtedly could be increased further by using Likert's method of scoring.

The immediate need in attitude research is an extensive analysis of the present Thurstone attitude scales, including the factor analyses of item intercorrelations on each scale for the purposes of purification, validation against more adequate criterion groups, and a more thorough study of the relationships between the various scales.

The writer can truthfully claim that this last statement is not merely an idle admonition to other psychologists to undertake this program, for it indicates a line of research upon which he is already well started.

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EVIDENCE FOR EDUCATIONAL VALUE IN DRIVERS' "CLINICS"

BY H. M. JOHNSON
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DeSilva, in his recent comment (**2**) on Johnson and Cobb's article (**6**) about statistical fallacies in certain propaganda for the establishment of drivers' "clinics," seems not to have discerned our purpose.

We did not intend to decide whether the test of skills and the personal advice that is based on the results are valuable or not; for on that question we are wholly in doubt, and therefore noncommittal. We sought, rather, to show that certain published findings of DeSilva and others (**1**, **3**) are worthless *as evidence* upon that question, and that the propaganda that has been based upon them is logically invalid.

In order to show that, by relying on a formal fallacy in such an instance as this, one can be misled into a very grave practical error, we presented some results of our own, compiled from (**8**), in which certain classes of drivers, selected according to the number of accidents that each driver sustained in one 3-year epoch, reduced their group-average number of accidents in another 3-year epoch to one-seventh of the former value. This reduction occurred without expert diagnosis and treatment; it is about as large as any that has yet been claimed as an effect of clinical treatment; and the probability that it is due to chance is practically infinitesimal. Thus, the sampling procedure, which is essentially the same as DeSilva's, may counterfeit an effect of treatment. We therefore proposed certain conditions of sampling and of statistical procedure that have to be satisfied before an effect that may be attributable to treatment can be evaluated or even detected.

DeSilva does not defend his logic, but reproduces some of his data and adds some that he has gathered later, presenting them as if they satisfied the conditions necessary for valid proof. Actually, they do not.

As Cobb and I pointed out, it is necessary to have 2 sample populations, both to be studied through 2 epochs. One sample is to be treated between the 2 epochs, the other not. Each sample must have had the same time, the same distance, the same number of operations, or some other uniform basis, in which to establish its "experience." Within one epoch of the common experience, the 2 samples must be *homogeneous with respect to the frequency distribution of accidents per operator*. To test their homogeneity, it is not enough that the 2 group-averages should be the same; one must make use of some such method as Pearson's test (**7**, **9**) to decide whether one may permissibly regard the 2 samples as having been drawn from the same population.

Suppose that we have applied Pearson's test to the 2 samples in both epochs. If (a) the 2 samples are not homogeneous in either epoch, it is at least difficult, and it may be impossible, to draw a valid conclusion about the effects of the intervening treatment. If (b) the 2 samples are homogeneous in both epochs, we must deny that the treatment was effective. If (c) they were homogeneous in one epoch but not in the other, a change has occurred that needs to be explained. One hypothesis to be tested is that the treatment produced the change, but many other hypotheses may be equally plausible, and therefore present the same claim to examination. If we eliminate all except the one that attributes the change to the treatment, then according to the direction of the change we may regard the treatment as being either good or bad.

In (**2**), DeSilva proves that his treated sample was not homogeneous with the untreated sample in the epoch that followed the treatment; he did not show that they were homogeneous before. Without this premise, his conclusion is invalid. To assume the premise, instead of proving it experimentally, is to beg the conclusion.

I should like to remove an erroneous impression which Dr. DeSilva derived from a paragraph in a popular article (**5**) which I wrote, but which the editors renamed. The paragraph followed a recommendation which was later carried out (**8**); namely, that a very thorough study be made of the official records kept by certain states to see whether unselected drivers sort themselves into liability classes as certain populations of commercial drivers had done. It began thus: "*If* we should find that general drivers fall into the same groups as commercial drivers do, *an* effective remedy"—namely, selective elimination—"*would be* evident." (Italics not in original.) By treating an hypothetical suggestion as if it were categorical, and by interpreting the word "an" as if it were equivalent to "the only," Dr. DeSilva convinced himself that I have "unbounded pessimism" about the possibility of bad drivers improving, and that I advocate selective elimination as "the only way to cure them." This feeling and this program are not mine. I disclaim pessimism, though I acknowledge skepticism about all plans of salvation until I can identify and judge their fruits. I can be convinced by genuine evidence, and if any is available, I should rejoice to see it clearly, fully, and unequivocally exhibited.

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BOOK REVIEWS

MUNN, N. L. Psychological development: an introduction to genetic psychology. Boston: Houghton Mifflin, 1938. Pp. xx+582.

This volume is, without question, the most comprehensive and satisfactory textbook that has appeared to date in the field of genetic psychology. Since the early influence of G. Stanley Hall it has been the American tradition to regard evolutionary as well as developmental psychology as falling within the scope of this field. In most cases, however, the presentation of the phylogenetic material has been brief, sketchy, and more or less dissociated from the developmental treatment. This seems hard to understand in view of the rapid advances that have been made in recent years in the domain of comparative psychology. It is encouraging to note that this fundamental defect in outlook and scope has been remedied to a large extent in the present volume. At least one-fourth of the text is devoted to facts and theories bearing upon the evolution of intelligence in subhuman types. Moreover, a serious attempt has been made to relate this phylogenetic background material to the later treatment of the developmental sequence in man. This broadened conception of the purpose and scope of genetic psychology has been presented very effectively in the first chapter, in connection with the discussion concerning the origins, problems, and methodology of this field.

The second chapter deals with the genetic mechanism in its ontogenetic phase in a clear and concise manner. This topic is further pursued in Chapter 6 in the treatment of prenatal activities. The facts and principles of genetics are graphically illustrated from selected experimental studies on animals. The author adopts a sane, conservative attitude toward the heredity-environment issue not only here, but throughout the volume.

Chapters 3, 4, and 5 are devoted to the general problem of the evolution of behavioral complexity in the phyletic series. While this section of the book is less complete than it might be, the material included has been well selected for the purpose in hand. The first chapter of this sequence deals with the phylogenesis of unlearned behavior, *i.e.* reflexes and instincts. The essential adaptiveness of these types of behavior is stressed by relating such activities to the basic physiological needs of the organism. In this connection, the more important concepts involved in evolutionary doctrine are defined and criticized. The notion of acquired somatic characters is rejected after due examination of various lines of evidence that have been offered in its support. The rôle of mutations in evolution is stressed in harmony with the prevailing views of the experimental geneticists. The author seems to be somewhat confused at this point, however, regarding the rôle of natural selection in evolution. Even Darwin did not suppose that natural selection produced genetic variations, but, rather, that it determined the survival of such variants. This would be just as necessary in the case of mutations as in that of the fluctuating or continuous germinal variations which Darwin stressed. In brief, natural selection operates only, and always, in the secondary episode of species-formation, regardless of the nature of the germinal variations arising in the initial episode. The other two chapters of this series trace the evolution of the basic sensory-neuro-motor processes, and learning and higher processes, in subhuman organisms. A chapter on the behavioral activities of early man might have been included at this point in order to complete the evolutionary picture. In spite of this omission, however, this section of the book provides an excellent phyletic background for the proper understanding of the behavior of the human child.

The genetic survey of the many aspects of human behavior from the period of birth onward occupies some ten chapters and is as complete as could be expected in a volume intended primarily for class use. It is interesting to note that the development of personality and emotional life is stressed quite as much as are motor activities and intelligence. The final chapter deals with the changes in personality which occur between adolescence and senescence. In most textbooks on genetic psychology this stage of the developmental cycle has been omitted altogether.

Although the treatment throughout the volume is critical in spirit, the style is lucid and interesting. It is a suitable book for the intelligent layman as well as for the more systematic student of genetic psychology. The volume is well illustrated, includes a comprehensive bibliography, and has an excellent index.

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LANDIS, C., & HUNT, W. A. (with a chapter by H. Strauss). The startle pattern. New York: Farrar & Rinehart, 1939. Pp. x+168.

In their introduction, the authors correctly state that significant findings often result from the careful investigation of relatively simple processes. The present volume demonstrates the correctness of this thesis. After a review of the literature on the subject, the authors explain the technique of the investigation, which was the same in outline for all of the special experiments reported. Essentially, this technique consists in the development of high-speed motion-picture cameras in such a way that accurate timing of the onset of the stimulus and of the temporal details of the response can be recorded.

The normal startle response, as previously described by Strauss, includes blinking of the eyes, head movement forward, change in facial expression, raising and drawing forward of the shoulders, abduction of the upper arms, bending of the elbows, pronation of the lower arms, flexion of the fingers, forward movement of the trunk, contraction of the abdomen, and bending of the knees. This is a basic reaction and is not amenable to voluntary control. A striking characteristic of the response is its speed. It usually has come and gone in less than one-half second.

Both repeated stimulation and knowledge of the stimulus may affect the character of the response, but in no simple way.

So-called "voluntary facilitation" of the response shows itself to be really a secondary duplication of the response. Changes in muscular tension and in bodily posture do not apparently affect the response positively or negatively. The pattern can be elicited by sudden strong stimuli of a nonauditory sort. By proper pairing of a previously inadequate stimulus and an adequate stimulus, the response can be conditioned. In general, the Moro reflex, which is dominantly an extension response, disappears in infants before the true startle response is established. The startle pattern in its typical form is found in primates and, indeed, in all mammals, but not in typical form in reptiles or amphibia. There is a tendency for the response to be exaggerated in catatonia and absent in epilepsy. In many other forms of mental illness the response is essentially normal. In the epilepsies the anomalies in pattern are so great as to serve as a diagnostic index.

The book also describes the pattern in the case of deaf subjects, adrenalized subjects, and hypnotized subjects. In the case of the latter subjects, the pattern can be better inhibited under hypnotic instruction than by voluntary control. The neural mechanisms of the response and the correlated physiological and psychological processes are also discussed.

In evaluating the book it may be said that it records a well-conceived and carefully executed series of experiments which give a definite and coherent picture of a simple and yet significant pattern of human behavior. It is a good piece of scientific work, well done and well reported.

LEONARD CARMICHAEL.

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GESELL, A., AMATRUDA, C. S., CASTNER, B. M., & THOMPSON, H. Biographies of child development: the mental growth careers of eighty-four infants and children. New York: Hoeber, 1939. Pp. xvii+328.

In this book four members of the staff of the Yale Clinic of Child Development have presented an instructive array of case histories showing a variety of trends of mental development in young children. The book is divided into a number of sections, each section being written by one of the authors and including a general statement and illustrative cases. The authors point out, repeatedly, that the cases presented in this volume are exceptional ones, selected from a much larger number because of the problems they present and because of their value in showing the types of cases in which prediction is uncertain. Nowhere in the book do they give figures indicating the extent to which these cases are atypical in regard to either the entire population studied by them or the selected groups discussed (such as foster children, twins, or children with physical anomalies). The value of the material in this book would be increased if representative data were included, with statistical descriptions of the mental growth trends in a normal or unselected group of children.

One of the points most strongly emphasized, especially by Gesell, is that simple numerical quotients, computed from single test scores, oversimplify a complicated set of developmental trends and hence are of little use for prediction of mental growth. However, the authors claim high predictive value for prophecies which are based on adequate clinical studies. In their opinion, clinical judgments should be based on: (1) division of the test scores into five or six categories of behavior; (2) information concerning socioeconomic status and ability of the parents; (3) a knowledge of the child's home environment; (4) the history of his physical growth and health; and (5) the evidence from previous tests concerning his rate of growth and general level of ability. That these clinical judgments actually give better prognosis than simple numerical test scores the reader is apparently expected to take on faith. However, even with this variety of information, the authors have not been able to predict the rates of growth for a large number of the children presented in this book.

Part I, written by Gesell, is devoted to thirty cases which he had described in an earlier publication (1928). He brings their growth curves up to date, usually by means of one or two tests given after a lapse of from four to five years. This is a selected group to begin with, including nine cases with some physical anomalies, such as mongolism, puberty praecox, cerebral palsey, and hypothyroidism. Three cases of feebleminded children were first tested after the (chronological) period of infancy. Of the remaining eighteen cases, the reviewer has attempted to make a summary which will give some notion of their general constancy of growth rates. To do this the range in IQ or DQ points, as reported by Gesell, was found for each child. These quotients are not always exact, as the figures were not always given. In three cases which were plotted on the same chart it was impossible to select accurately the figures which represented a particular child. For fifteen children the mean range of IQ (or DQ) points is 30. If the three additional cases (whose curves appear to be very consistent) are arbitrarily assigned an average range of 0 IQ points, the mean range for the eighteen cases would still be 25. These means are far larger than the 5 to 7 points ordinarily reported for single retest scores for school children. The greatest change is 60 points for one child whose quotient dropped from 70 at the ages of five to twelve months to 10 at thirteen years. (In this case the clinical practice of weighting heavily the most mature items proved to be misleading, and a more conservative rating during the first year would have given a somewhat better prediction.)

Gesell, in summarizing these cases, says: "In the whole series of thirty diversified specimens there is no instance in which the trend of mental growth appears whimsical or erratic" (p. 102). It is true that all of the cases show mental growth over at least a part of the period, and none shows actual deterioration. In some cases there is a fairly consistent trend in rate of growth. Where shifts in quotients occur, Gesell looks for explanations and is usually able to find them. But there are shifts which he cannot explain—and even those changes for which he finds a reason could not usually have been predicted.

The semilogarithmic curves employed to represent development emphasize the continuity of growth and mask variations in relative scores. These variations are most effectively masked in the earlier ages when small deviations in rate of growth bring about large displacements in a child's position in his age group. Fluctuations in scores are passed over lightly as not being reliable indications of the child's trend of development. This is very likely true in many instances, and is an indication of the need to repeat tests at successive ages if one would find a child's "true level" of ability. By "prediction" Gesell means only the possibility of determining whether a child will be feebleminded, borderline, dull normal, normal, or superior. Obviously, prediction within narrower limits would fail in most cases, although, clinically, it is probably not important to make finer distinctions. Still, for these thirty cases of Gesell's, prediction is uncertain even within these wide limits.

In Part II, groups of cases are presented by the different authors. Thompson points out the greater difficulty of detecting superiority in infancy, giving four cases, three of whom did not follow the expectancy from scores made during the first year. Castner presents two cases of delayed language development, one of whom overcame the handicap with training. He presents six cases in which preschool prediction of reading disabilities was fulfilled. His criteria for prediction are interesting and appear to be well substantiated. Castner then presents ten cases of irregular mental development. In a general statement his position is seen to be in certain respects very different from Gesell's. He states: "Although the behavior growth processes have tremendous power to overcome obstacles, the underlying mechanisms are complex and delicately integrated, and the headlong rapidity of their development in the early years of life makes them particularly susceptible to temporary disturbance during that period. As a result, we encounter among infants and pre-school children more fluctuations in development . . . than are found in later years when the organism has become more stabilized . . ." (p. 173). The position taken in this section is that irregularities in mental growth are due almost entirely to hindering factors which prevent a child from realizing his full potentialities; that only rarely does a child "make deceptively high scores that are not confirmed by subsequent examinations" (p. 174). The problem for the clinician, then, is to detect a child's highest potential ability. These statements are not supported, however, because the illustrative cases given are admittedly atypical, and their relative incidence is not stated. Gesell next gives three illustrative cases of the mental growth of prematures, repeating for the most part what he has said on this subject in previous books. He follows with three pairs of twins, and raises the question whether early retardation is typical of monozygotic twins. Amatruda gives a series of thirteen cases with physical complications. In some the physical condition proved a handicap to mental development; in others it did not. She finds that it is often impossible to predict the extent to which a child will overcome the handicap, even where there is injury of the brain. She follows with a section on children in foster homes. Most of the nine cases given here are influenced in their test scores by changes in their environment, but this is not true of all of them. She stresses the need for caution in making prognoses of mental growth from measures taken in infancy.

In the concluding chapter the authors point out the *individuality* of children's growth careers. They say that "mental growth is both labile and stabile . . . a constitutional core of characteristicness determines the way in which the individual will meet new situations and incorporate them" (p. 305). Because of the lability of mental growth, environmental factors influence its trend to some extent.

The case-history method should prove to be very fruitful in clarifying the qualitative aspects of growth and in bringing to light factors which influence trends of growth. As for the present series of cases, it makes interesting reading and illustrates well many of the generalizations made by the authors. However, the arguments will often prove unconvincing to those not already convinced. The reviewer would willingly accept many of the statements concerning the development of intelligence and would doubt, or take exception to, others; but they are little more than textbook statements which the pupil is expected to read and accept without verification. The principal conclusion that one can draw from these case histories is that early prediction of intelligence from even the Gesell type of clinical appraisal very often fails. In cases where it is important to know a child's mental level we must make frequent tests and rely primarily on recent tests of his ability. It appears in this book that Gesell, while protesting to the contrary, in effect admits that diagnosis in infancy of future mental status is relatively uncertain—except in cases of mongolism, untreated cretinism, or where there are other well-recognized stigmata of feeblemindedness.

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CONKLIN, E. S., & FREEMAN, F. S. Introductory psychology for students of education. New York: Holt, 1939. Pp. v+557.

A good many authors of textbooks on educational psychology seem to be of the opinion that a discussion of some of the principles and factual data of general or introductory psychology is educational psychology. Strange as it may appear, they seem to think that general principles are dynamic and force their own applications. Occasionally, the point is made that the principles and hypotheses of general psychology are not equally applicable to professional educational problems, and that it is necessary to evaluate and make selections carefully in organizing a psychology for teachers. It is also suggested at times that applications here and there to methods of teaching, curriculum organization, and classroom management make a particular organization of subject matter of an educational character. Such ways of thinking, however, do not characterize the authors of the textbook under consideration. They state:

> "The authors believe that the present wide interest in child study and in adolescence particularly in normal schools, teachers colleges, and schools of education, is highly desirable so far as our schools and the preparation of teachers are concerned. At the same time the authors hold that the study of

children and adolescence is the more significant the more it rests upon a knowledge of essential psychological materials and concepts" (p. v).

Apparently, child psychology and adolescent psychology are the distinctly professional courses in educational psychology. One might presume that the psychology of the school subjects could be added to these two courses. The introductory course "points" the student for thinking in the purely professional fields. It supplies him with principles that are generally useful. This point of view is, in itself, a distinct contribution—it emphasizes the value of the general principle and at the same time recognizes the place of purely professional training. The reviewer, however, is inclined to question, on psychological and logical grounds, such a division of subject matter. Is it not true that the best type of training is that which is general in its operation, and that a knowledge of child and adolescent development is of much more general value than any teacher or group of teachers in public educational institutions could make of it? That is why teachers colleges, normal schools, and other professional schools, under able and expansive guidance, have found it necessary in meeting the needs of their constituents to provide a type of general training which arts colleges have neglected or disregarded. Of course, the mind is plastic and responds to a variety of influences. It can be compartmentalized, but the disadvantages of compartmentalization are obvious. The bona fide psychologist should be the first person to recognize this—for academic and practical reasons. The authors have struck an important principle in psychology, but they have failed to extend it far enough.

The problems discussed are those commonly recognized as of educational importance. Professor Conklin is responsible for seven chapters dealing with (I) "Introduction" (The human adult and the human infant); (V) "Feelings and Emotions"; (VI) "Sensory Sources of Knowledge"; (VII) "Perception"; (XI) "Thinking"; (XIII) "Handicaps to or Disturbers of Development"; and (XIV) "Fatigue, Sleep, and Drugs." Roman numerals indicate chapter numbers. Professor Freeman wrote eleven chapters on (II) "Original Behavior"; (III) "Original Behavior and Motivation"; (IV) "Personality"; (VIII) "Remembering and Forgetting"; (IX) "The Nature of Learning"; (X) "Conditions and Results of Learning"; (XII) "Attributes of Behavior" (Attention, Interest, Attitude); (XV) "Intelligence"; (XVI) "Measurement of Intelligence"; (XVII) "Individual Differences, Their Nature and Causes"; and (XVIII) "Points of View in Psychology."

One might gain a fairly accurate notion of the method of approach to the various problems by examining two or three discussions. "Original Behavior and Motivation" are discussed in the thirty-eight pages comprising Chapter III. The student of education is told that hunger, thirst, elimination, and rest are motives that cannot be denied. The sexual motive, on the other hand, does not have the same imperative character. It may be sublimated. Parental behavior, curiosity, and the desire for recognition are also discussed. The problem of incentives is presented in four and one-half pages. Group rivalry, encouragement and discouragement, and Lewin's general interpretation of the effects of punishments and rewards are discussed. Mixed motives and reflexes are mentioned. The problem of motivation and incentives, however, is dealt with again in Chapter IX, on "The Nature of Learning," where the goal is recognized, and in Chapter X, on "Conditions and Results of Learning," where the work of Prescott, on "Emotions and the Educative Process," is quoted. There is also quite a lengthy discussion of "interests" in Chapter XII. The entire discussion seems to be fairly complete from the standpoint of the psychological principles involved, although the reviewer wonders whether the student of education will recognize that a clear understanding of the reasons for action, regardless of the kind, and a determination to do the thing involved are most effective volitional determinants. The most difficult thing to do in writing a general psychology is to achieve a high degree of integration. It is possible, however, that the type of differentiation which the authors have achieved is even more difficult.

"Transfer of Learning" is dealt with in six pages. The studies of James and Thorndike and Orata are given as references. The conclusion, "The foregoing results signify that if the maximum transfer effect is to be achieved, it is necessary that the relevant methods of learning and doing and that the general principles involved in the situation be made explicit and emphasized," is a good example of the careful judgment exercised by the authors. If there is a difficulty it seems to be one of exposition. However, there is a chapter on "Thinking" immediately following. Concept formation, generalization, and abstraction are mentioned.

The chapter on "Fatigue, Sleep, and Drugs" is of general interest and might be found in a textbook for any type of preprofessional training. However, the treatment of each of these problems is rather brief. Fatigue is disposed of in six pages. The student is given only the very early work of Thorndike and Offner as references for further study. The problem of sleep is discussed in five pages, and the references are general. Three pages are devoted to the effects of alcohol, and in these three pages the case is made and closed against the use of alcohol. The effects of caffein, tobacco, and marihuana are described in four pages. Studies by Hull, Miller and Miles, and Switzer are given as references on the effects of caffein. No mention is made of the effects of drugs related to caffein. Hull's study on the effects of tobacco is given as a reference, although the case of tobacco is left in the air, unless one can accept the verdict that no case can be made. Several specific studies on marihuana intoxication are mentioned. Apparently, marihuana has assumed educational importance.

The discussion of retention and forgetting seems to emphasize: (1) the retention of nonsense material, (2) reminiscence, (3) methods of measuring retention, (4) conditions of forgetting, and (5) distortions of memory. The discussion of the conditions of forgetting refers only to studies by Van Ormer, Bassett, Meltzer, and McDougall. These are some of the more important studies prior to 1930. It is somewhat surprising that so little attention is given to a discussion of the effects of meaningfulness and overlearning on forgetting and retention. The Ebbinghaus curve is important, but the beginning student of education will need it put in a proper setting.

The references for students and teachers are to a few of the "best contemporary presentations of the subjects discussed." The following names appear frequently throughout the book: G. W. Allport, Boring, Cannon, Conklin, Dunlap, F. S. Freeman, Garrett, Gesell, Hull, James, Klineberg, Koffka, Köhler, Langfeld, Murchison, Thorndike, J. B. Watson, Weld, and Woodworth. To be sure, there are other references, but the absence of some is very conspicuous. This type of bibliographical research is followed by a few writers of textbooks, and there are some who do not even go to the trouble of quoting their general sources. From a practical point of view this might be a very poor policy because a good many persons like to have their research used; they seem to feel more or less like the authors of textbooks. General summaries, however good they might be, tend to obscure individuals and probably reduce the sale value of the book. However serious this problem might be, there appears to be a question of much greater importance concerning the advisability of using general references. Principles issue from facts, and facts issue from records made during experiments. Would it not be of considerable pedagogical value to describe outstanding studies in such a way that they could be understood, and show how the principles have issued from the data? This type of presentation could then be followed by a deductive process in which the principles were applied to practical situations. The authors have given an application now and then. Analysis of this sort would make textbook writing more or less difficult, but a textbook of such a character would assist a great many teachers who openly admit they need such assistance. It would also tend to eliminate a great deal of authoritarianism which sophomores dislike.

Any final judgment on a book such as this is a matter of personal opinion. Many will find the organization perfectly satisfactory and will enjoy using the book as a text. The beginning student of education can only profit by the guidance that such an outline affords.

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Grant, V. W. Psychological optics. Chicago: Professional Press, 1938. Pp. 240.

This concise manual is designed to fulfill the needs of the student and the practitioner who are professionally concerned with visual problems. After an introductory treatment of general principles, both psychological and physiological, the author deals with the general theory of visual perception, the perception of space, attention in relation to vision, color vision, and visual illusions.

The reader will not find in this book any basically different viewpoint from that of the familiar treatises on "physiological optics." A "psychological optics," one might expect, would emphasize that there are specifically psychological problems of vision which are to be disposed of neither by physiological nor by epistemological theorizing, and which demand, for their treatment, a thorough and precise phenomenological analysis of the facts, absolutely unbiassed by any notions as to the *causation* or *philosophical status* of the facts. To the recognition of this principle, insisted upon by Hering, we owe every fundamental advance that has ever been made in the psychology of vision. This principle is far from being generally acknowledged by those who approach the problems of vision from the medical side. There is still a strong inclination to force facts into the pre-established molds of the Berkeleyan or the Helmholtzian or some other philosophy, or into the molds of current physiological theory, before they have received the respectful examination that ought to precede all intellectual manipulation. The present book, unfortunately, simply follows the old pre-Hering tradition.

The following sentence may serve as a sample of the book's attitude toward psychological problems: "When, as was pointed out in Chapter IV, a content of consciousness is discovered which could not, by its nature or by consideration of the character of retinal stimulation, be furnished by the eye, it must represent an expression of the processes of associative memory" (p. 171).

Except for one sentence concerning the Hering color theory, not even the name of Hering occurs in the book. The developments that have been inspired by the phenomenological attitude—to mention only a single example, the important work on space and motion perception that has been done by M. H. Fischer and other physiologists of the Hering school—are not taken into account. Indeed, the author cites no publications in any foreign language, in spite of the fact that the problems of visual psychology have been, for many years, on the whole, only a subject of desultory interest in English-speaking countries.

The author sometimes goes so far as to treat in an *a priori* fashion matters that are properly (and easily) to be settled by experiment—for instance: "It seems doubtful . . . . whether a fact unknown to the observer, namely, the difference between homonymous and heteronymous images, can validly be classed among the visual signs of distance. A more plausible suggestion is that the location of double images is judged on the basis of such monocular cues as size, overlapping, etc." (pp. 136 f.). But if one consults, for example, Koffka's article in *Psychologies of 1930,* one will find that there are experiments which conflict with this "suggestion."

In the reviewer's opinion, the best part of the book is the interesting and informative discussion, in the third chapter, of the motor behavior of the eyes.

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De Gramont, A. Problèmes de la vision. Paris: Ernest Flammarion, 1939. Pp. 282.

This book deals with the structure of the eye, refractive defects, spatial perception, and color vision. The point of view is more physiological than psychological. Except for a rather bizarre interpretation of retinal response in terms of "piezoelectric resonance," the book contains little that is novel or original. The style is clear and lively.

D. M. PURDY.

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MANOIL, A. La psychologie expérimentale en Italie. Paris: Félix Alcan, 1938. Pp. viii+489+16 planches.

This volume summarizes the contributions of Father Agostino Gemelli and his co-workers, Galli, Gatti, Pastori, and others, at the Catholic University of Milan, which he founded, together with the laboratory for psychology and biology. Gemelli first completed his studies in medicine, specializing in neurology with Golgi. Following his period of theological studies as a Franciscan monk, he worked with Golgi, Verworn, and others in physiology. Later he obtained a doctorate of philosophy, founding a neoscholastic journal of philosophy. He then worked with Kiesow, Kulpe, and Kraepelin in psychology and psychiatry, devoting himself primarily thereafter to experimental psychology, but including work on psychology of aviation and vocational psychology. He posits an autonomous science of psychology independent of biology and philosophy, but assuming a psychophysical parallelism, which may be approached from different angles. Present-day Catholic psychologists should find his philosophical conceptions quite helpful as a basis for their psychology.

In Part I, "Studies on Emotions," Manoil reviews all of the major theories of emotions, checked by his own biological experiments, and concludes against any purely peripheral or cerebrophysiological explanation in favor of his own psychophysiological theory of central origins. His later introspective experiments attempted to determine the functional significance of emotions in relation to fundamental necessities of life. Organic concomitants and movements of attraction or repulsion related to organic needs were usually reported and considered to be instinctive.

In psychophysical measurement of two-point tactile discrimination thresholds, unfilled space was found to be overestimated—just the opposite of results of vision. The optimal number of points in filled space, durations, and practice effects was determined. He also considered these experiments as contributions to the psychology of thought and introspective methodology. The method of equivalents (or average error) was later employed extensively in the study of tactile space and weight discrimination as influenced by various skin areas, imagery, blindness, fatigue, attention, etc. Very extensive data are given for numerous related experiments.

Part II includes researches of Gemelli and collaborators after 1921, in the laboratory of psychology of the Catholic University of Milan, where Gemelli founded the first chair of psychology in Italy. The main problems are contributions to the theories of perception, psychophysiological study of language, the selection of aviators, and other studies in applied psychology.

In studies of perception Gemelli emphasizes the influence of purposive sets, a point which is related to his neoscholastic philosophy. All perception is a compromise between the sensorial data presented and the active attitude (biopsychological needs) of the subject. For instance, Gatti explains illusions in terms of the set with which one approaches the perception, *e.g.* when considered as a simple distance or as a broken series [emphasizing separation (fr., *disjonction*; it., *distacco*)] or as background [emphasizing nearness (fr., *voisinage*)]. According to the principle of *disjonction,* where spatial relations are indefinite their perception occurs in such a manner that the maximum distances are accentuated. Similarly, according to the principle of *voisinage,* the minimum distances are decreased in apparent size. The attitudes adopted are said to follow the law of least action or economy of effort. For instance, all distortions (illusions) have the rôle of permitting an immediate perception of the whole.

Four stages of perception are posited: (1) recognizing presence, (2) appearance of form, (3) grasping of meaning, and (4) naming the object. In meaning, the function of the image is the unification of sensorial data. An intellectual element is also suggested, somewhat in line with the Würzburg school. Gemelli and his co-workers object to the law of *Prägnanz* of Gestaltists and revert to the older emphasis on the importance of attention and previous experience. Contrary to Gestalt, perception is not an immediate datum, but is the result of an active elaboration and integration of sensory data by the observer. These conclusions were derived, in part, from introspective reports on the formation of perceptual patterns during the development of the stimulus figure by an ingenious slow motion picture controlled by the subject. The use of difficult (complex or unfamiliar) figures served the same purpose as slow motion pictures in differentiating stages of perception.

Galli confirms the above finding on perceptual stages by experiments on successive phases of peripheral vision. He also confirms the fact that we may perceive either by parts or by the whole. In studies of apparent movement in combinations of visual, tactile, and auditory stimuli, the bearer of apparent movement was most commonly visual, with tactile next. Combinations of more than three fields did not produce apparent motion.

Minor studies on perception in twilight vision showed that under certain conditions the figure tended curiously to fuse with the ground. The conditions favoring this result included diminishing light, secondary figures in the ground, and motion of the ground. Masking of figures was difficult unless the accessory figures were richer, so as to include the first figure in the new whole.

Electroacoustic studies of language with Pastori employed oscillographic recording along with ordinary observation and *analyzed* the physical bases of vowels, consonants, syllables, etc. in isolation, combination, and under conflicting conditions. The physical structure of sound elements was found to vary with a wide variety of changes in context. Sixteen plates of sample oscillograms are included. Manoil regards these experiments on language as among Gemelli's most important contributions.

Early studies on aviation in 1916 included those on the perception of rotation in horizontal and vertical planes, together with studies of pulse, respiration, etc., under decreasing air pressure. Although the tests are said to be used at present in the Italian army, no data are given concerning their validation in relation to criteria of actual flying skill. Following minor studies of difficulties in flying, Gemelli recommends the formulation of tests on perception, attention, and emotional stability, but recognizes the nonexistence of a general motor ability.

Smaller studies, including data, are reported on developing tests for vocational selection in a spinning mill and a shoe factory. Tests involving work samples were found to be more closely related to success than were tests of attention, reaction time, tapping, precision of movement, etc.

At the end of the book is a bibliography of about 450 titles and an index.

E. L. Clark.
R. H. Seashore.

Northwestern University.

Munn, N. L. A laboratory manual in general experimental psychology. (Rev. ed.) New York: Prentice-Hall, 1938. Pp. viii+286.

This revised manual is comprised of sixty-five eclectic experiments which, according to the author, roughly parallel the sequence of Dockeray's *General psychology* (Rev. ed.). However, the experiments can be used with any text. Like all such manuals, it will not please every instructor, but the large number of experiments included in it will allow considerable latitude of choice to meet individual instructors' needs.

The reviewer's chief criticism is that the difficulty level of the various problems is uneven. Part of this unevenness arises from a lack of completeness of instructions in some of the problems. For example, Experiment 6, on "The Limits of Tonal Sensitivity," calling for the use of Appun's lamella and Konig's bars, does not include specific instructions on counterbalanced orders. After instructing the experimenter to increase the frequency of the lamella until a tone is heard, the manual explains: ". . . note the frequency of vibration at this setting. Now reverse the procedure. Beginning with a frequency of 24 cycles, gradually reduce the number of cycles until a tone is no longer heard. Average the two determinations. This average will approximate the subject's lower threshold for tonal sensitivity." This would seem to be simplification to the point of almost misinforming the student as to the meaning of the threshold concept. It would seem more informative to use a detailed set of instructions which would better indicate the psychophysical methodology in determining the threshold. The description and instructions for this same experiment in Guilford's *Psychometric methods* (p. 118) illustrate the point and the nature of the criticism. The instructions for many of the experiments give little justification for the statement in the Preface, except at a very elementary level, that "much emphasis has been placed on types of control required in psychological experimentation."

The experiments quite adequately cover the field of experimental psychology, one or more being devoted to various aspects of motor functions, nervous system and nervous mechanisms, sensory sensitivities and characteristics, psychophysics, drives and motivation, fatigue, perception, emotion, learning, imagery, intelligence, personality, attitudes, abilities, suggestion, and testimony. There are a few relatively unimportant gaps represented by a lack of demonstration of size and color constancies, and insufficient coverage of psychophysical methods.

The learning experiments are especially noteworthy and deserve considerable praise for their selection. These eighteen learning experiments range through conditioned reflexes and animal learning to human verbal and rational learning and are well formulated to bring out the salient features of the process. The inclusion of several animal experiments on drives, activity, and learning seems a very worthwhile addition to the experimental sequence on learning.

The manual makes a generally successful attempt to utilize simple, inexpensive, and usually available equipment. To help those who may not have the equipment on hand, an Appendix gives sources of laboratory supplies that will be needed, together with prices, catalog numbers, and suggestions for fabrication for each experiment.

Each experiment is accompanied by an unusually good list of references (generally at a more advanced level than the experiments themselves seemed to have intended) and a tear-out page of questions with spaces for short answers. The questions attempt to bring out the general significance of the problem and seem, in the reviewer's experience with the manual, to succeed fairly well.

Each page of the manual is perforated for tearing out and punched for binder storage. It has seemed to the reviewer, who has used some of the experiments for his own classes, that page space for answers to questions is wasteful of paper and the student's time. The good student will have more to say than the space allows, and the poor student is satisfied with just enough material to fill the space. This criticism is probably a matter of personal preference on the part of the reviewer and may not be considered an undesirable feature by other instructors.

In spite of some unevenness in the completeness of the instructions and in difficulty, as mentioned above, nearly all instructors of experimental psychology will find the manual useful. The wide range of available experiments to select from, the simplicity of the setups and apparatus requirements, and excellent references make it adaptable to the needs of practically any elementary experimental psychology course. In any case, whether the manual is used as intended or not, it should be an excellent source of experiments, references, and discussion topics. It is particularly worth noting that no systematic point of view, other than objectivity, is represented, yet it would be possible by selecting particular experiments to demonstrate at least one aspect of nearly all contemporary systems bearing on experimental psychology.

JACK BUEL.

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BURLOUD, A. Principes d'une psychologie des tendances. Paris: Félix Alcan, 1938. Pp. 430.

No small amount of psychological literature induces the impression that as psychology grows older it takes on the characteristics of the sick man of the sciences. Not a few writers refer to the crisis in psychology and proceed to diagnose its disorders and prescribe remedies. Some point out that the trouble with psychology is that it harbors too many schools and cults. Others find the difficulty in its fundamental principles —for example, it is too physiological or is not physiological enough. Still others complain that psychology is too abstract and remote from the concrete facts of everyday life. Again, some writers believe psychology suffers from being too discrete and lacking in such unity as a central core of individuality might provide.

Burloud finds psychology generally infected with troublesome diatheses. There is discrete sensationalism; there is biological or physiological determinism, bare behaviorism, general mechanism, and the static formism of the Gestalt psychologists.

The author's therapeutic is even more interesting than his diagnosis. According to him the remedy for psychology is contained in the prescription that we should return to Biranism. That is to say, psychology should base itself upon such principles as the intentions, tendencies, idées forces of Maine de Biran. To go back to the study of a spiritual subject —namely, to an activity which senses, perceives, imagines, and thinks— will not only give psychology a directing idea, but also a set of primordial dynamic principles. Though Burloud insists upon the subjective and spiritual character of psychological phenomena, he is careful to make plain that he does not imply that psychological life should be independent and separate from the organic, but that the guiding principle should be that of the forces and powers indicated.

Needless to say, the forces and tendencies in their causal efficacy and evolution not only (in the opinion of the author) account for all psychological phenomena, but likewise make available sources of metaphysical principles. Thus, psychology, unlike the material sciences, can establish no laws, but, on the other hand, it has the great advantage of knowing true causes and authentic action. Moreover, it knows these immediately and adequately by an original intuition upon which is founded a positivistic and incontestable method of investigation and interpretation (pp. 426–427).

Despite the fact that Burloud's theses speak for themselves, he attempts to implement them by bringing them into juxtaposition with recent experimental procedures and findings. It may not be supererogatory to point out that the primary significance of this book lies precisely in its exemplification of how mystical doctrines are scientifically implemented.

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Indiana University.

LUND, F. H. Emotions: their psychological, physiological and educative implications. New York: Ronald Press, 1939. Pp. xiii+305.

After a brief introductory chapter in which the identification of emotions is discussed, this book proceeds through a treatment of the neuroglandular basis of emotion, cardiovascular and respiratory changes, gastrointestinal and sexual changes, metabolic and skin changes, the development and control of emotions, emotion and motivation, and ends with a summary which integrates the whole and suggests further lines of research for the future. The book is primarily physiological in tone. Although Lund defines emotion as "a strongly affective state involving diffuse somatic reactions and rather widespread, centrally aroused, visceral changes," he considers the visceral changes most important and devotes most of his time to them. The neglect of the conscious aspects is striking and is based on Lund's belief that the introspective approach has brought confusion to the field. His identification of what he calls the "mentalistic" approach with a strict dualism may seem a bit anachronistic to some readers.

The physiological emphasis in the book is a healthy one and may help to counteract some of the previous neglect of this approach. The line between general physiology and the special physiology of emotion, however, is not clearly drawn, and there is included in Lund's treatment a great deal of physiology which may be considered as extraneous to emotion. As an example we might mention the two pages of text and the page diagram dealing with the passage of food through the digestive tract. There are about nine pages on normal metabolism and its measurement, and no more than that on its changes in emotion. The chapter on emotion and motivation contains much material on the effects of praise, reproof, and knowledge of results on performance. Unfortunately, this material is included at the expense of further citation of other more relevant material and of a further discussion and interpretation of those researches cited.

Nor will all psychologists agree with some of Lund's neurological interpretations. The frank espousal of F. H. Allport's theory that pleasantness depends upon the action of the craniosacral branches of the autonomic nervous system and that unpleasantness depends on the action of the sympathetic branch will irritate many. The uncritical acceptance of the thalamic localization of emotion is against current trends. There are occasional original hypotheses that are tossed in without adequate defense.

One might also wish that its bibliography were a bit more "up-to-date." There are relatively few titles from the last five or six years; references are made to abstracts of spoken papers where the complete paper has been printed; and the *Foundations of experimental psychology* is quoted when the later *Handbook of general experimental psychology* would have been better.

All in all, it is not a reference book for the scholar. It seems, rather, to be aimed at the undergraduate student and to be available either for a special course on emotion or as collateral reading in other courses. At this level, its oversimplification of problems, the extraneous physiological material introduced, and the overabundance of nonessential illustrations may be virtues rather than a handicap.

WILLIAM A. HUNT.

Wheaton College.

Dunbar, H. Emotions and bodily changes: a survey of literature on psychosomatic interrelationships 1910–1933. (2nd ed.) New York: Columbia Univ. Press, 1938. Pp. xl+601.

The new edition offers twenty-five pages of new Introduction, and a new bibliography of 106 examples from the 1933–1938 literature illustrative of points in the new Introduction. We are given here the promise of the new journal, *Psychosomatic Medicine,* which has now appeared and which will contain, in early numbers, surveys of various subjects originally dealt with in *Emotions and bodily changes.* It is pointed out that this new project will afford greater opportunity for presentations by specialists in the various fields. Thus may be overcome some of the insuperable obstacles to comprehensive treatment where space is limited. Special consideration is given in the new Introduction to muscle tension, inhibition, specificity of emotions, changing fashions in mortality and morbidity, and to the need for centers of psychosomatic research.

Chester W. Darrow.

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University of Illinois, Chicago.

Sandiford, P. Foundations of educational psychology: Nature's gifts to man. New York: Longmans, Green, 1938. Pp. xv+464.

The author tells us that this book is "the first of a series designed to give a general survey of the foundations of educational psychology." It deals with Nature's gifts to man. The second volume will deal with the psychology of learning. We are not told what other volumes in the series are planned.

This plan to cover systematically the whole ill-defined field of modern educational psychology is an ambitious attempt for any present-day psychologist. Thorndike did it about twenty-five years ago with his famous three volumes. Those three volumes influenced the arrangement and content of almost all books on educational psychology written since that period. In the meantime, however, an enormous amount of work has been done in every area touched upon by Thorndike and in a great many new fields. No one, since Thorndike, has attempted a detailed, systematic treatment. Now comes one of Thorndike's pupils, and we are curious to know how much, or how little, he will include in his total plan. At present we have his first volume and, judging by this, we may expect a lucid and careful treatment of whatever he undertakes to do. For the book is well written and excellently organized. Despite the great amount of research upon which it is based, the argument proceeds clearly from the beginning to the end of each chapter. The author has mastered his material thoroughly and presents clearly the divergent points of view with regard to the many controversial issues that exist. He shows where gaps in our present knowledge exist and points to places where research is needed to fill them.

What, then, does this first volume of "foundations" contain? It is primarily concerned with what man inherits. He inherits a certain type of bodily and nervous organization which tends to respond to the outside world in certain ways (reflexes, instincts, and emotions), and this inherited constitution is the basis for his intelligence and personality.

The Introduction deals briefly with the methods employed in psychological research, with scientific method in general, and with the various schools of psychology. After this general orientation the author takes up in the first chapter the problem of heredity and environment, emphasizing the fact that they are correlative factors and not opposing forces. The presentation here is very detailed, going into the theory of the gene, describing such matters as linkage and crossing over, giving an account of Mendel's experiments and theory. I feel there is too much detail here for the student of educational psychology and much that has little bearing upon the psychological studies that follow and form the real core of this chapter. This is, of course, a matter of opinion. At any rate, the material is clearly presented and well illustrated by diagrams. After this the author presents the main data with reference to the question of the inheritance of psychological characteristics. He gives many tables showing the original findings of various workers. He evaluates fairly these findings, indicates where gaps in our present knowledge occur, does not take an extreme stand in interpreting them, and reminds us continually that "heredity and environment are correlative factors."

Chapter II shows how the psychologist became interested in individual differences and discusses the findings with reference to race, sex, maturation, and so on. Extreme individual differences in man lead the author to discuss the problems of the blind, the deaf, the feebleminded, and the gifted. No two psychologists would agree as to just what, of all the extensive research in this wide field, should be or should not be included in such a chapter. I would not devote five whole pages to the detailed results of the Terman and Miles masculinity-femininity scores, but otherwise I think the selection and emphasis of the material is excellent.

Chapter III deals with the physiological bases of behavior. The emphasis here is upon the dynamics of the processes rather than upon the anatomy of the nervous system.

Chapter IV discusses nonvariable or unlearned behavior. Tropisms, reflexes, instincts, and emotions are the main topics. The conditioned reflex of Pavlov and the work of Watson and others are adequately presented. A brief discussion of the various theories of emotions and some of the most important experimental studies of the emotions are included here.

The next chapter covers the nature of intelligence and intelligence testing. There is a very good account of the various theories of intelligence, a brief history of the rise of intelligence testing, a description of our present-day tests, and a discussion of the problems of growth of intelligence, constancy of the IQ, distribution of intelligence, and the like. It is a very good chapter, and it is difficult to see how anyone could have done better in the space at the author's disposal. All the main points are there.

The last chapter is devoted to personality. Our author threads his way skilfully through this morass without becoming bogged down at any one place. Due attention is given to theory. Typology is discussed;

mental hygiene is touched upon, and the various techniques of measurement are described. There seem to me to be fewer presentations of the actual data of personality measurements and experimentation in this chapter as compared with the data presented in other chapters.

These, then, are the six chapters of the book. They cover six important areas in educational psychology. They are full of the main data in each area, presented by means of tables and graphs. There are excellent bibliographies at the end of each chapter. I am sure this book will prove one of the best textbooks in educational psychology. Sandiford is no partisan for this or that theory, trying to convince the reader to accept some hypothesis or other. He tries to be fair to all theories, but puts his main energy into presenting those facts which have been established in educational psychology. Each teacher who uses his book can ride his pet hobbies to his heart's content, but the students will get a solid foundation by following the text. The author's aim to encompass thoroughly the foundations of educational psychology has been excellently attained.

R. PINTNER.

Teachers College, Columbia University.

FREEMAN, F. N. Mental tests: their history, principles and applications. (Rev. ed.) Boston: Houghton Mifflin, 1939. Pp. xi+460.

This volume is a revised edition of Dr. Freeman's book of the same title, published in 1926. The revision represents no radical change in point of view. Rather, it consists in the expansion of certain chapters, in the addition of new material and of new references designed to bring the treatment up to date, and in the omission of certain special topics. The new book is forty-three pages shorter than the old.

Comparison of the new and old books, chapter by chapter, will give the clearest notion of where changes have been made. Chapters I and II in the new book, dealing with the history, present status, and early work with mental tests, are essentially the same as corresponding chapters in the old book. Paragraphs have been inserted here and there and the references at the end of Chapter I brought up to date. Chapter III, "The Application of the Correlation Method," contains a new section on factor analysis. Chapter IV, on age scales, contains a description of the 1937 Revision of the Stanford-Binet Scale, plus several minor changes. Chapter VI in the old book, "The Early Development of Point Scales," becomes Chapter V in the new, with no changes. Chapter VI in the new book, "Survey of Point Scales," is Chapter VII in the old book. Additional material has been included in this chapter covering recent tests, and the bibliography has been expanded. Chapter VII, "Tests for the Analysis of Mental Capacity," is "old" Chapter V; there are new sections dealing with tests of music and art and with clerical, mechanical, and other abilities. Chapter VIII, which describes personality tests, is also Chapter VIII in the old book. New sections have been added, describing recent tests of interests, attitudes, and introversion-extraversion; and the references have been brought up to date. Chapters IX, X, and XI, which deal with the techniques and theory of mental tests, are the corresponding chapters in the old book, plus considerable new material on primary abilities, item selection, constancy of the IQ, and accuracy of scores. Chapter XII, on the tabulation of test results, corresponds exactly to Chapter XII in the old book. Chapters XIII and XIV, which deal with the educational uses of mental tests, are permutations and combinations of the material in Chapters XIII and XIV in the old book. Several changes and additions have been made. The section on the constancy of the IQ in Chapter XIII has been expanded; and new material on ability grouping has been added to Chapter XIV. Chapter XV, "Interpretation of Mental Tests," is "old" Chapter XVII. Several sections have been rewritten and recent developments included; for instance, the Iowa studies of the preschool child and recent work on foster children are discussed. Chapter XVI, "The Nature of Ability," is "old" Chapter XVIII, plus new material on factor theory and minus some of the discussion of Spearman's two-factor hypothesis. Chapters XV and XVI in the old book ("The Application of Mental Tests to Vocational Guidance and Selection" and "The Relation of Intelligence to Delinquency") have been omitted from the revision.

Any author who attempts to summarize a fairly extensive field in a brief space must, of necessity, omit much which others will judge to be important. Dr. Freeman can hardly be criticized on the grounds of omissions alone. But he can, I think, be criticized (1) for inadequate treatment of several important subjects and (2) for frequent failure to bring his treatment up to date. Consider, first, the much discussed topic of factor analysis, to which several sections are devoted in Freeman's book. The discussion throughout is extremely general and not always accurate. For instance, Freeman writes: "In the last dozen years several forms of analysis which are different in detail but based upon the same fundamental principle and interchangeable mathematically have appeared" (p. 81). He then mentions Thurstone's and Kelley's methods, which are not alike in principle nor interchangeable mathematically. Freeman fails to mention Hotelling's pioneer work, upon which Kelley's more recent methods are based. And he omits, also, the important experimental studies of factors by Schneck, Anastasi, DuBois, Bryan, Schiller, R. L. Thorndike, J. W. Dunlap, and others. On page 245, in discussing those factors which have been suggested by various workers, Freeman attributes to Holzinger a list of factors which should be credited to Spearman, and omits entirely Spearman's general factors of perseveration, oscillation, and will. No inkling is given of the fact that factors and factor theory have been severely criticized by several able workers. Surely, the student deserves to be told of the early criticisms of Spearman's two-factor theory by Godfrey Thomson (whose name is misspelled in the Index) and of the general criticism of factors as "mental elements" by R. C. Tryon and G. W. Allport.

Of several other topics Freeman's new book gives a superficial and sketchy account. In discussing the kind of age-progress curve necessary to yield a constant IQ (pp. 291–297), Freeman points out (as in the old book) that either a straight line with increasing scatter as we go up the age scale or a logarithmic curve with constant scatter will give a constant

IQ. But the student is not told just what the most probable form of the mental growth curve is; and the graphic material in the old book dealing with growth in various mental functions—which might help—is omitted. On pages 295–296, Freeman makes a bow to the Heinis growth curve and the P.C., but no mention is made of Thurstone's and Thorndike's mental growth curves in which the attempt was made to measure changes with age in equal units and from an absolute zero. In Chapter X, in the section on "Problems relating to the section and organization of the items of a test," there is no mention of item analysis and only the briefest treatment of test construction. The work of Sandiford and Long, of Lindquist and others is not mentioned. In fact, the material is essentially the same as it was in the first edition—thirteen years ago. The rather futile discussion of Burt's regression equation (pp. 401–403), repeated from the old book, is certainly not calculated to enlighten the student as to the factors which operate in determining Binet mental age. The argument hinges essentially upon one's definition of such factors as school achievement, "reasoning," and age, as Freeman himself points out in a footnote (p. 401).

Chapter VIII, on personality tests, devotes nine pages to the Downey Will-Temperament Test, although Freeman comments (all too correctly!) that ". . . the prevailing opinion among psychologists is that the test does not measure any real characteristics of the personality" (p. 214). Only one-half page is given to tests of introversion-extraversion; and the discussion of tests of neurotic tendencies, attitudes and opinions, and the like is so brief that the student will get from the book alone little more than a conversational acquaintance with these measures.

It seems rather remarkable that, after thirteen years, Chapter IV in the revised edition, "Age Scales: The Binet Scales," should need only one additional reference (the Terman-Merrill book, *Measuring intelligence*) in order to bring it up to date. Chapter V, "The Early Development of Point Scales," makes no mention of the revisions of Army Alpha by the Psychological Corporation and by F. L. Wells or of the revision of Army Beta; the latest reference is 1931. Careful perusal of Chapters XIV and XV, dealing with the application, interpretation, and uses of mental tests, may well leave the reader wondering whether anything important in mental measurement has happened since 1926. In the five and one-half pages devoted to racial differences, for instance, only one of the six references cited is as recent as 1926; in the six pages which deal with mental test differences shown in various localities and by groups of diverse natio-racial origin, the most recent reference (there are seven) is 1924. Army Alpha, which was more of a test in 1926 than it is now, is still Freeman's stand-by when differences among occupational groups and variations in intelligence associated with schooling differences are to be discussed. F. S. Freeman's and Anastasi's comprehensive treatments of individual differences are omitted along with all recent research. In fact, the student must certainly gather that the last thirteen years have been marked by stagnation in the field of mental measurement.

On the whole, Freeman's book strikes me as a hasty revision of his old book and as a not very thorough review of the field of mental measurement. Professor Cubberley, in his Introduction to *Mental tests,* writes: "The past dozen years have been important ones in the field of mental testing . . . The author in his revision has fully covered these more recent developments and studies and theories, thus making his text again, as it was originally, a comprehensive presentation of the field of mental testing up to the time of issue . . ." Unfortunately, this editorial boost is more optimistic than accurate. In a brief introductory course for undergraduates, Freeman's text will suffice. But for the serious student of psychometrics more and heartier fare should certainly be provided.

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Columbia University.

GRIFFITH, C. R. Psychology applied to teaching and learning. New York: Farrar & Rinehart, 1939. Pp. xiii+650.

Those who are constantly searching for a better elementary text in educational psychology must not assume that *Psychology applied to teaching and learning* is merely a further development or minor revision, under a new title, of Griffith's *An introduction to educational psychology,* which was published in 1935. It is true that both volumes are organized around the central concept of growth and that both are ostensibly addressed to students in introductory courses, yet two volumes could hardly be more dissimilar. The earlier volume, in spite of its discoursive, wordy style and occasional thinness, was a scholarly contribution, actually addressed to rather mature students, heavily ladened with discriminating citations of the literature, and full of illuminating, and even profound, passages. The more recent volume is drastically written down to the level of the much less mature student. Each chapter is introduced by an italicized "Note to the Student." Each of the many sections within a chapter begins with a "Section Preview" and ends with "Summary," "Reading," and "Exercise." The suggested reading is usually a single chapter reference. In the earlier volumes there were no charts; now there are fifty-six, many of which are not relevant to the topics they are supposed to illustrate. In order to be perfectly clear, the author is repetitious. Analogies between physical and psychological phenomena are overworked. The superficial treatment of such topics as mental faculties, original nature, instincts, and heredity in the introductory chapters cannot be other than confusing to the intelligent student. In brief, the volume bears many earmarks of the conscious effort to make things easy for the student while failing to achieve simplicity, directness, and clarity of exposition.

The sixteen chapters cover the topics of growth, maturation, learning, habits, perception, concepts, attention, motivation, emotion, thinking, attitudes, personality, and individual differences. Of these, Chapter III, on the facts and principles of growth, is the most outstanding. The development in this chapter (especially pp. 108–129) of the author's nine "principles" of growth is a model of straightforward exposition at an elementary level and represents, in addition, a stimulating and original analysis.

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CAGE, M. C. Reading in high gear. New York: Harper, 1938. Pp. 347.

"*Reading in High Gear* offers assistance and direction to those boys and girls who have not yet attained their reading power." It "is in no way limited to short reading exercises, but from the beginning and all the way through, the aim has been to develop life-long reading habits." This modest aim apparently is to be achieved by study of 109 pages of "lessons in *How to Read*," and use of 105 pages of "practice material with directions and tests, designed to promote the different kinds of reading skills and to appeal to different interests." The student is to keep a record of his score on each of the 103 "tests" and is admonished to "see it grow!" Such a procedure may provide a device for motivating the student but will be of little aid to the teacher, since the tests have apparently not been standardized, as no norms are given.

There is one laudable note of caution in the book. "It is designed to help high school students of normal intelligence who have reading difficulties." Training other than that offered by the book is advised for cases of extreme difficulty, and "physical handicaps should be referred to experts."

The source from which the particular remedial method offered by the book is drawn is a mystery to the reviewer. No reference, published or otherwise, is given as to whether the good "life-long reading habits," or reading habits of any other duration, have ever been achieved by the use of the book in preliminary form or a special course in remedial reading with a similar approach. Nor are principles derived by competent workers in the field offered as a basis. Even the usual "long experience in the field" is not offered as justification. It may well be that the author has had astounding success with her approach over such a period of years that she feels relatively sure "life-long habits" can be achieved by its use. Otherwise, it seems that she ought to be sporting enough to admit that she wants others to bet their money and time on a pipe dream. Certainly, if psychology and experimental education have done nothing else, they have at least indicated that a remedial method could be tried out under controlled conditions before it is published—that is, if its proponent cared to go to the trouble.

EDWIN E. GHISELLI.

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BUROS, O. K. (Ed.) The 1938 mental measurements yearbook of the School of Education, Rutgers University. New Brunswick: Rutgers Univ. Press, 1938. Pp. xiv+415.

This text is a comprehensive description and evaluation of recent mental tests published in Great Britain and the United States. The term "mental tests" as used in this title includes "aptitude, educational, intelligence, personality, and psychological tests, questionnaires and rating scales." The test descriptions and evaluations were compiled by 133 coöperating test reviewers.

The "1938 Yearbook" is an outgrowth of two earlier publications. The first of these was a mere bibliography of educational, psychological, and personality tests of 1933, 1934, and 1935. The 1936 edition added a critical part in the form of a book-review-excerpt section.

The 1938 edition gives the following information (when this information is available) about each test listed: title, group for which the test was constructed, date of publication, individual or group, number of forms, time required, outline, publication references, and cost of test.

The descriptions of tests are classified under the headings: (a) achievement tests, general and specific, attitudes and opinions, character and personality, intelligence, group, individual, interests, study habits and skills, vocations; (b) mental measurement books; (c) research and statistical books; (d) regional testing program reports; (e) periodical directory; (f) index of publishers directory; and (g) index of titles and names.

This text is not the kind one would sit down and read from cover to cover. Instead, it is an excellent reference book for all those who are interested in the construction and use of any kind of psychological test. Here, the classroom teacher, the college teacher of tests and measurements, the clinical worker, the personnel director, and the research worker will find in concise, convenient form the pertinent facts about the recently constructed tests, as well as brief abstracts and references to practically all of the critical material about tests that has been written in the last few years. The *1938 mental measurements yearbook* is the most comprehensive compendium in the field, and it is to be hoped it will be followed up by later editions, if not annually, at least biennially. It constitutes a very complete psychological abstract in this one field.

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DOLLARD, J., DOOB, L. W., MILLER, N. E., MOWRER, O. H., & SEARS, R. R., in collaboration with C. S. Ford, C. I. Hovland, & R. T. Sollenberger. Frustration and aggression. New Haven: Yale Univ. Press, 1939. Pp. viii+209.

"This study takes as a point of departure the assumption that aggression is always the consequence of frustration." Aggression is dependently defined as "that response which follows frustration, reduces only the secondary frustration-produced instigation, and leaves the strength of the original instigation unaffected." Frustration is independently defined as "that condition which exists when a goal-response suffers interference." Again, aggression is independently defined as "an act whose goal-response is injury to an organism."

On the basis of these definitions the assumption and correlates following the assumption are applied in their general psychological reference to "Socialization in America," "Adolescence," "Criminality," "Democracy, Fascism and Communism," and to "A Primitive Society: The Ashanti."

This collaborative effort from the Institute of Human Relations represents an admirable production. The precision and clarity of expression may serve as a model of scientific exposition. The application of this simple assumption to a wide range of problems surrounding the socialization of the individual and to the reactions of an individual in society at large has been well thought through. The general notion of widening the scope of the application of psychological principles to the problems of contemporary civilization is well done.

The concept that aggression is always the consequence of frustration is, at the same time, one which simplifies thinking while raising any number of new problems, some of which appear to fit the formula. That this monograph should lead to a series of experiments seems both obvious and desirable.

As a production in social psychology this work has much to commend it. It deals not only with unitary and simple experiments, but does attempt, and usually successfully so, to place the implications of those experiments in their proper social reference.

CARNEY LANDIS.

Columbia University.

BRUNTZ, G. G. Allied propaganda and the collapse of the German Empire in 1918. Stanford Univ.: Stanford Univ. Press, 1938. Pp. xiii+246.

This volume is No. 13 in a series of publications from the Hoover War Library of Stanford University. It is written from the point of view of the historian and therefore describes in great detail and with ample documentation the organization and techniques by means of which the Allied propagandists were able to demoralize both the civilian and the military population of Germany during the last war. The writer attempts to measure the effectiveness of this propaganda by reference to the official reports of the Psychological Subsection of the United States Army, the rather clumsy counterefforts of German authorities, the mutinies in, and desertions from, the German Army, and the letters of German soldiers. At the end he concludes that, although "propaganda was only one of the many weapons used in the war to combat the enemy," it "cannot be denied" that it was "an important instrument of warfare."

Scholarly research of this kind, which appears two decades after the events it analyzes have become history, has little explicit connection with the theories of either psychology or social psychology. Naturally, it provides exciting and valuable anecdotes with which students can be stimulated as they are being taught whatever principles of mass psychology their instructor is able to formulate within the loose framework of psychology. At the same time, the work of an historian of propaganda inevitably is challenging to the social psychologist who is interested in systematizing his particular discipline.

The question that excites everyone as Europe plunges forward toward another war in 1939 is the extent to which events of 1914 are about to be repeated. Will the same propaganda appeals be employed again to weaken the morale of the enemy? An actuarial reply may be filled with errors because people and their society have changed in the meantime. The only really constant element in the propaganda situation is the people who are affected. If these historical materials can be reinterpreted in terms of psychological processes which have been exposed and conceptualized in the gentler atmosphere of the clinic and the laboratory, then an insight may be obtained into mechanisms that have been and will be at work. Mr. Bruntz, for example, describes the "internal conditions of Germany" during the latter part of the War and rightly calls them "an aid to propaganda." This means that Germans were psychologically prepared to succumb to the appeals which reached them in such ingenious ways; this means that propaganda helped to release impulses that had already been aroused; this means that a similar procedure can be employed in the next war only under similar psychological conditions which, in turn, may or may not be related to the social conditions of 1917–1918. If the reasons for successful war propaganda are expressed psychologically, more of the *if's* can be specified, and practice and prediction can be guided by a knowledge of the variables that are involved. The enemy will be demoralized a second time—not by repeating the same propaganda but by using propaganda that, being adapted to psychological conditions, will tap other drives and attitudes in more or less identical ways.

From this survey it is possible to recognize once again a severe limitation with which social psychology is confronted. Mr. Bruntz's data are not psychological data, since they are confined to the stimuli (the documents) and to the generalized responses of unspecified people. The responses themselves, moreover, have not been measured except in a suggestive, qualitative manner. War propaganda, then, which is a "psychological" weapon, is really not accessible to the psychologist, since the people involved are dead or changed and since in a future war anyone desiring to conduct a psychological investigation on the spot probably will be accused of espionage. All that can be accomplished through an analysis of this type of propaganda, it seems, is to verify or illustrate established principles in a rather subjective way. In view of his inability to be strictly and thoroughly empirical, the social psychologist is compelled to deduce his explanations as rigorously as possible and to lay bare the precise reasoning he has employed.

LEONARD W. DOOB.

Yale University.

HOOPES, G. G. Out of the running. (With a Foreword by E. A. Doll and with Clinical Notes by W. M. Phelps.) Springfield, Ill.: Thomas, 1939. Pp. xvii+158.

This autobiography has as its central theme the manner in which the author has been able to adjust to a rather extensive intracranial birth lesion and to build an enriched and, on the whole, satisfying life with those factors at her disposal. Her expressed purpose in writing the book is twofold: first, to make some contribution to the scientific literature of her defect; and, second, to offer to others who may suffer from the same handicap some courage and comfort. In addition, she has produced a document which may serve as a source book for students of the abnormal.

According to a clinical note by Dr. Winthrop M. Phelps, the lesion is probably extrapyramidal, a hemorrhage into the basal ganglia. As a result, there is marked motor involvement, including constant athetoid movement of the arms, poor control of both arms and legs, and total inability to speak. On the other hand, there is no demonstrable mental defect, and Miss Hoopes is considered definitely superior by those who come into contact with her. Likewise, there is no evidence of involvement of the frontal areas, and the occasional quirks of temperament which she cites are presumably produced by her circumstances of life.

Miss Hoopes was fortunate in that she was born into a fairly large family of some financial standing; for that reason she has been afforded opportunities which might not be available to another similarly handicapped person. She has had interested and skilled teaching and the companionship of at least one member of her family at all times; various ingenious devices for her comfort and convenience have been provided. As a result, she has been able to receive a great many impressions from the world about her, and she has built up an acquaintance with the fields of literature, art, music, and religion, together with a knowledge of current events. She has developed the ability to communicate with others, although tediously; at times her literary style reflects her impatience at being unable to record as rapidly as she thinks, but she expresses herself clearly and forcefully.

This book makes no pretense as a psychological text; it is therefore not in our province to cavil at the expression of beliefs such as those of prenatal influence and mental telepathy. However, the emphasis on such beliefs makes it inadvisable to put it into the hands of an unguided student. On the other hand, the student of the emotions will find much interest in her discussion of her emotional life.

From his background of long study of the intelligent physical defective Dr. Edgar A. Doll has contributed a Foreword to the book.

HELEN MARSHALL.

Stanford University.

ROSETT, J. The mechanism of thought, imagery, and hallucination. New York: Columbia Univ. Press, 1939. Pp. x+289.

The fundamental thesis of Rosett's book is that the mechanisms of thought, imagery, hallucination, and emotional tone can be accurately conceived only on the basis of the ". . . physiological and anatomical fact that the cessation of activity of a link in the nerve pathway results in a flaring up of the function of the nerve link which succeeds the former in the normal direction of nerve conduction."

Diminution of activity of the sensory receptive areas releases the associative centers from ordinary inhibitions, making possible thought. Thalamic centers may also be released, with consequent increase in the emotional factor of vividness. If release continues to the point that the associative centers are greatly affected, the activity of thought gives way to the production of uncontrolled images and, finally, to hallucinations.

If the associative centers are, in turn, completely disabled, consciousness is lost, and the cerebral motor centers are released. If this wave of extinction continues to the motor centers in the brain stem and cord, the muscles become flaccid, and the tendon reflexes disappear. All phenomena in general are in keeping with Jackson's Law of Dissolution.

Rosett supports his thesis by the following observations: Destruction of a sensory receptive area results in hallucinations of the affected sensory modality. Similar phenomena may appear if the function of a receptive area is extinguished during an epileptic seizure. Thought is possible only if an active state of attention is maintained, and "in any one state of attention most of the function of sensory reception is therefore in abeyance." The onset of sleep is characterized by three stages: the activity of thought, activity of imagery, and hallucination. These stages are correlated with hypothesized progressive functional extinction of the nerve pathway from the receptor toward the motor centers.

Rosett's position is not supported by all clinical data. The visual sensory receptive area has frequently been destroyed or excised without hallucinations in the receptive field being reported. Indeed, often the patient is unaware of any change in the visual field. Penfield has reported cases of hallucinations following occipital lobectomy, but these were apparently initiated by focal epileptic centers anterior to the tissue excised.

Rosett cites the allied phenomena of attentive states and sleep stages as proving that hallucinations, imagery, and thought are release phenomena, instead of the result of irritating or stimulogenic effects.

At present, however, the neurological correlates of attentive states and sleep stages are unknown. There is no proof that the primary projection areas are depressed before the associative centers.

The psychologist is certain to be disappointed in the completely inadequate psychological references in Rosett's book and the naïveté of the various theories dealing with psychological subjects, particularly learning.

H. F. HARLOW.

University of Wisconsin.

HORNEY, K. New ways in psychoanalysis. New York: Norton, 1939. Pp. 313.

This important book is a development of the systematic implications of the author's earlier book, *The neurotic personality of our time*; in it she examines in detail both her differences from, and her agreements with, the classical Freudian position.

The kernel of her viewpoint, as in the preceding volume, is that neurotic disturbances are conflict reactions, due to incompatible strivings for a security that has been threatened by specific factors in the environment. This common-sense viewpoint cuts the ground from under most of the chronically unacceptable Freudian formulations, while leaving the factual observations and the major concepts (repression, conflict, transference, etc.) as valuable as ever. Thus, if neuroses are, basically, modes of defense against insecurity, sex becomes demoted from the source of all motivation to a department in which, as in other departments, insecurity fears are reflected; the Œdipus complex is neither universal nor basically sexual, but represents a kind of clinging for protection;

narcissism becomes one of several possible types of defense—the one in which deflationary invasions are combatted by attempted self-inflation; penis envy is no longer anatomically determined, but is a symbolic formulation of the reaction of women to cultural factors threatening their security; the destructive impulses are not due to any generalized death instinct, but are attacks upon a hostile environment; the period of childhood is only one part, though possibly the most important one, of the character-forming history of the individual; transference is not only a reactivation of former dramas, but is a sample of present attitudes; culture is not simply a repetition and outgrowth of long-ago happenings, but promotes or guards against neurosis according to the insecurities which it fosters; the "ego" is a neurotic structure rather than a psychic "instance"; anxiety is a result of inescapable threats to the safety devices; the "super-ego" is another "neurotic trend" (*i.e.* safety device), and guilt feelings are a similar anxiety reaction; masochism is another of the main types of safety device, coördinate with narcissism, *i.e.* it is the kind of defense which operates through dependency and unobtrusiveness. There are also excellent chapters on the fundamentals of psychoanalysis (which will be a boon to the harassed student who has wondered what, amid all of the obvious fantasies, really *is* important); on the basic premises of Freud's thinking, *e.g.* biologism in its less sophisticated nineteenth-century forms; and on the implications for therapy, which in the author's view is fairly active and oriented to the present character structure and relationships rather than toward an hypothetically determining past.

It hardly needs to be said that such a radical overhauling—so similar in its results to what sympathetic psychologists have been saying for a long time on a mainly dialectical basis—coming from a person highly trained in the classical tradition and tempered by the flow of fifteen years' case material through her consulting room is of revolutionary significance. We suppose that Dr. Horney will be "read out of the Party" in several quarters, but, since recent events have transplanted the psychoanalytic movement bodily to democratic landscapes, there seems little doubt that her views will have an opportunity to influence theory and practice greatly; they have, in fact, a considerable similarity in attitudes to those of the common-sense, nonmystical, American and English analysts of the type of Sullivan, Hill, Sharpe, and Isaacs.

It is just possible, however, that Dr. Horney has not gone quite far enough. One of the bases of her opposition to the Freudian theories is their genetic quality; understood as she understands it (which is probably an accurate reflection of the way Freud understands it)—namely, as a conviction that later experiences are simple repetitions of earlier ones, with the corollary that recall of the earlier will abolish the latter—this opposition is no doubt justified; but the choice of the term has derailed valuable portions of the argument. It is hard to understand how a satisfactory account of human development (and maldevelopment) can be given which is not genetic in the correct sense of the word; and the failure to give a generalized genetic hypothesis of development is the book's most conspicuous omission. (Freud gave one, though it is, for the reasons Dr. Horney has cited—among others—unacceptable.) Such an hypothesis needs development at three points, and at none of these does Horney reach maximal clearness: (1) Some account should be given of just what happens when the child is made to feel "helpless and defenseless and . . . [to] conceive the world as potentially menacing." In psychological terms, we need a clearer account of the characteristics of the stimuli and their mode of operation which lead to conflict and frustration. It is true, but not adequate, to say that "disturbances in human relationships become the crucial factor in the genesis of neuroses." (2) A more systematic account should be given of the modes of reaction to these conflict stimuli. Dr. Horney gives the impression of stopping just short of this; the following can be gleaned from her pages: A child threatened by his environment may fight, which is not a "neurotic trend"; he may theoretically (but not usually practically) escape, which is also not a "neurotic trend"; he may develop a supplicating or clinging reaction (masochism, Œdipus complex); he may develop compensatory self-inflation (narcissism); or he may attempt rigid compliance with the demands upon him (perfectionism, superego). Psychologists would like to know such things as: Is this a complete list? Do these reaction types overlap? What is their basis? How could their determination be at least partially objectified? (3) A clearer theory of therapy is badly needed. Dr. Horney's account is that examination of the neurotic character structure and its relation to the patient's adjustment makes its inconsistencies and disadvantages clear to him, modifies his anxiety, and thus makes it possible for him to give up his "neurotic trends" as unnecessary. This may be the best that can be done at the moment, but we still need to know how this kind of additional stimulation comes to induce this kind of reaction in the patient; the matter is of far more than academic significance, for, evidently, if we knew how certain stimuli modify anxiety we could apply them earlier and perhaps prevent its formation. To summarize this whole matter, Dr. Horney is vaguely, but not clearly, aware of the general concept of learning, which she conceives in sociological terms; she understands that "environments" and "human relationships" have a major part in "forming character," but she has not taken the complete step of seeing the organism as played upon continuously by stimuli of various sorts, being constantly modified by them, and so presenting a different set of habit patterns (including visceral and ideational ones) to be modified by successive stimuli. Among the consequences of such a thoroughgoing psychological view would be the concept of therapy as additional stimulation, in which, no doubt, the principle of extinction of inhibitory habits would play an important part. Another would be the better evaluation of the terms "neurotic" and "normal," which now bear traces of their medical origin, in Dr. Horney's argument, in a sort of recurring differential diagnosis: the neurotic person has such and such characteristics, as distinguished from the normal, who has something else. But if we adopt a generalized concept of the sort indicated, some of the interest can be diverted from the drawing of boundaries, to become available for description of stimulus-response histories of all kinds, from "normal" through "neurotic" to "psychotic"; and we shall presumably find these to be of all degrees of individual complexity, but continuous and with a few unifying threads running through them, which threads would be nothing less than the basic principles of affective psychology.

The author is moderately unsympathetic toward attempts to think physiologically about psychoanalytic phenomena, and recognizes correctly that Freud's "economic" and "dynamic" thinking, in spite of figures of speech, is so oriented. This lack of sympathy would seem to close doors unnecessarily; granted that the subjective quality of anxiety or desire is *sui generis,* there are evidently important physiological bases or at least concomitants; and the nature of libido as Jung understands it, *i.e.* as general drive, is surely a problem which can be ignored by no careful approach to affective problems. Again, the conditioning approach seems even more promising.

A chapter deserving of special mention is that on the rôle of culture in neurosis (we should prefer to say, in line with the considerations above, in adjustment). It contains a careful analysis of just what factors in a culture can be considered pathogenic, with applications to our own culture. For example, there are "factors . . . which create emotional isolation, potential hostile tension between people, insecurity and fears, and a feeling of individual powerlessness"; these include the system of individual competitiveness, gross inequalities (of opportunity as well as possessions), and exploitation. Again, "there is the question of how our culture impairs self-confidence . . . The individual's existing limitations . . . should not impair his self-confidence; but by virtue of the fact that external limitations are only slightly visible, and particularly by virtue of the ideology that success is dependent on personal efficiency, the individual tends to accredit failures to his own deficiencies. . . Furthermore, the individual in our culture is as a rule not prepared for the hostilities and struggles that are in store for him." This type of analysis seems to be new (though foreshadowed in the "Neurotic Personality") and pregnant with social and educational implications of the highest significance.

The book may be counted as perhaps the fourth major systematic critique of Freudian psychoanalysis, not including Parthian shots by the secessionist schools; it is, however, the first which has proceeded from a basis of experience and careful observation, and the first which has tried judiciously to assess strengths and weaknesses and their causes. The job has probably been done well enough on the negative side so that no one need spend further time or energy on it; better, an admirable job of reconstruction has been started, which can be forwarded with entire self-respect by every school of human thought that is seriously concerned with affective phenomena.

RAYMOND R. WILLOUGHBY.

Brown University.

HOOTON, E. A. Crime and the man. Cambridge, Mass.: Harvard Univ. Press, 1939. Pp. xvi+403.

This volume is a semipopular presentation of voluminous anthropometric measures of 17,680 individuals, from sane civilians to insane criminals. More technical treatment of the same data has been published in a separate volume, and more is promised.

All the statements of the book, when "gestaltized," result in "a declaration that the primary cause of crime is biological inferiority" (p. 130). Hooton points out that different animals behave differently because of their respective organisms; for example, human mental processes and behavior differ from those of the anthropoid apes because of differences inherent in the organisms. He does not want to be interpreted to mean that any particular organic structure is directly causally related to any specific behavior.

The bulk (10,953) of Hooton's measurements were made with prison and reformatory inmates. He tags as "stupid" the objection that such samples of criminals are the failures of the criminal population.

As a criterion of significance of the numerous differences in physique, he used three times the probable error of the difference in means—a none-too-stringent requirement. As a result, he found that, with thirty-three measures of cranial, facial, metric, and morphological features, the criminals of Massachusetts, Tennessee, Kentucky, Texas, North Carolina, Wisconsin, and those of Arizona, New Mexico, and Colorado grouped, each had patterns of significantly different features. In this study he counted only the "Old American criminals," after Hrdlička's definition of "Old Americans." He has illustrations of front and side views of what he calls "mosaics," depicting the heads of the various State criminals on the basis of their significantly different characters.

Then there are the different types of crime for Old Americans, for which schematic mosaics are presented. First-degree murderers, for instance, are significantly different from other criminals in excesses of stature and jaw breadth, to mention two out of their ten divergences out of a possible thirty-nine. Second-degree murderers significantly exceed other criminals in five items, two of which are excess of chest depth and deficiency of head height.

Hooton relates his nine body-build types of Old Americans—all combinations of three degrees of weight and height—to types of crime. As examples, short-heavy men rank first in rape, tall-slender men are first in robbery and second-degree murder, tall-medium-weight men are first in forgery, fraud, and extractives, and tall-heavy men lead in first-degree murder.

Another sample of the kind of differences Hooton found is that robbers had significant excesses over other criminals in nine items, such as attached ear lobes and iris diffused in pigment. Now, only 11.74% of robbers had such an iris, as compared with 8.60% of the other criminals. Thus it was that he found six robbers out of 414 who had no robber characteristics, and one robber had seven. No robber had eight. Hooton heads the chapter in which these data are given "The Will-o'-the-Wisp Criminal."

It is impossible here to do justice to the multitudinous differences between Old American criminals and civilians, new American criminals and civilians, various races among white Americans with each other, the criminal and civil insane, and negro and negroid criminals and civilians.

The important point is that many specific differences were found, no matter what comparisons of groups were made.

Hooton emphasizes that comparisons of criminals and civilians should be made only within an ethnic group. When such comparisons are made, the criminal is of inferior physique. It would be interesting to know whether crime would be interpreted as an absolute or a relative biological inferiority. If mediocre physique in an inferior ethnic group would be equivalent to inferior physique in a superior ethnic group, in each case would criminals be found among the inferiors of their group? Or again, how many of the differences he found are aesthetic inferiority rather than biological inferiority?

The book is entertaining—in fact, so much so that one wonders whether the author might not, in the privacy of a little circle of friends, say: "And those people took me seriously!" On that possibility, all is forgiven, and he will be taken seriously, in spite of his wisecracks about himself and his work, which give some readers an impression of protective facetiousness.

MILTON METFESSEL.

The University of Southern California.

WINN, R. B. Scientific hypnotism: an introductory survey of theory and practice. Boston: Christopher Publishing House, 1939. Pp. 168.

This small, popularly written book, though not as scientific as it might be, will not harm the laity, because it is "proper" in seeking to explain the phenomena in naturalistic terms and it is "safe" in demanding that hypnosis be used only by professional people. On the other hand, it contains little which is not available elsewhere. Because of its insistent scientific claims and because of the possibility that others may be interested in the way that the author—a partially converted philosopher—handles the well-known data, a more detailed criticism follows.

Part I, "Theory," presents a mélange of mentalistic and organic concepts, eventuating in this definition of suggestion: ". . . it is a prestige-and-faith relationship in which the practician uses his advantageous position to influence by suggestion the subject's autonomic nervous system, in order to effect desired bodily inhibitions and excitations and to condition his mind accordingly" (p. 86). All psychoanalytical concepts are contemned, along with any other functional concept. But, as one might surmise from the definition of suggestion, the explanation of phenomena wavers between organic and functional concepts, as occasion demands (cf. pp. 66 and 134).

Part II, "Practice," tells how to hypnotize, discusses dangers, describes "oneirosis" (a light form of hypnosis), gives details of a group experiment, and lists applications in psychology, medicine, and education. "Conclusions" lists nineteen propositions summarizing "the positive results attained in the course of my studies"—which are nothing new. "Oneirosis" is new only in name, if that, and is described without reference to Hadfield's hypnoanalysis, Schultz's autogenic training, etc.

While flaunting his scientific approach, the author uses only a dozen of the 150 studies reported in the last ten years, nor does he deal with those (along with some other older studies) rigorously. He does deal, of course, with much of the stock in trade of hypnotism. The author's inadequate orientation in the field may be observed from the following illustrations: Mesmer attempted to raise "suggestion [*sic*] to the level of practical science" (p. 15); hypnotic study bids for acceptance "into the family of recognized sciences" (p. 16); hypnosis is "merely a state of high suggestibility" (p. 43); yet "the cooperation of the subject usually decreases with the deepening of the trance" (p. 124); earlier successful time-evaluation experiments are cited but not later unsuccessful ones (p. 109); G. H. Estabrooks [allocated to psychiatry] is twitted for explaining hypnosis by reference to the unconscious—"what is it, mere naïveté?" (p. 77); the work of C. L. Hull and associates [allocated to medicine] is characterized as follows: "But here as elsewhere the research is being conducted largely at random and by guesswork, without sufficient directives of a comprehensive theory and without adequate controls of experimental technique. In psychology and education, on the other hand, even these signs of interest are lacking" (p. 131); and finally, reaching the climax of disorientation, the author uses O. H. Mowrer's note reporting diminution of tonic rigidity, with repeated hypnotizing of a rooster, to show that there is "some danger that the efficacy of posthypnotic suggestion will be reduced by repeated sittings" (p. 111).

This disregard for rules holds for the un-English style of the book, a style partly explained by the statement made on the jacket that the author came to this country in 1923 from Finland. But such an explanation hardly excuses the editor and the proofreader for countenancing such expressions as the following: "Finally, the day has arrived, and I was ready" (p. 159); "they let imagination break the boundaries of the common sense" (p. 54); "I do not mind even to try my own hand" (p. 99).

This book illustrates some of the difficulties with which hypnotism has been beset both in its experimental aspect and in its presentation in print, even at the hands of those who, like the author of the book under review, are apparently trying hard to do it a good turn.

PAUL CAMPBELL YOUNG.

Louisiana State University.

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NOTES AND NEWS

DR. ERICH GOLDMEIER, PH.D., M.D. (Frankfurt), recently of the Jewish Community Hospital in Berlin, has been appointed research associate in psychology at Wheaton College, Norton, Massachusetts, where he will assist in research in the psychological laboratory.

DR. THOMAS R. GARTH, since 1930 professor of experimental psychology at the University of Denver, died on April 20 at the age of sixty-seven years.—*Science.*

DR. ALBERT A. CAMPBELL, of the department of psychology at Northwestern University, has been appointed to a Social Science Research Council Fellowship for the coming year. He will probably spend the year at Cambridge University, working with Professor Bartlett on problems in social psychology and anthropology.

THE following have been appointed Consulting Editors for the *Journal of Experimental Psychology,* beginning with the July issue of that journal: Charles W. Bray, Princeton University; Elmer K. Culler, University of Rochester; Clarence H. Graham, Brown University; Joy P. Guilford, University of Nebraska; Francis W. Irwin, University of Pennsylvania; Donald G. Marquis, Yale University; Arthur W. Melton, University of Missouri; and Carroll C. Pratt, Rutgers University.

THE honorary degree of doctor of science has been conferred upon Dr. Raymond B. Cattell by the Senate of London University for researches on the nature of temperament, its manifestations, measurement, and dependence on environmental and hereditary influence. Dr. Cattell is now associate professor of genetic psychology at Clark University.

THE May issue of the *American Journal of Sociology* was devoted to a symposium upon: "The Relation Between the Individual and the Group." It contained contributions by investigators in psychology, psychiatry, sociology, and cultural anthropology. The contributors in psychology included: R. S. Woodworth, J. E. Anderson, J. F. Brown, K. Lewin, W. E. Blatz, and F. H. Allport.

THE Board of Directors of the National Committee for Mental Hygiene have announced the appointment of Dr. George S. Stevenson as medical director, succeeding Dr. Clarence M. Hincks, who has asked to be relieved of his duties, except as part-time field consultant, in order to give more time to the work of the National Committee for Mental Hygiene of Canada, of which he is general director and founder. H. Edmund Bullis, who has served as executive officer of the National Committee and as assistant to Dr. Hincks, is continuing on the staff as part-time business manager.

Dr. Henry J. Arnold, professor of psychology and director of the Division of Special Schools at Wittenberg College since 1925, has been elected president of Hartwick College, Oneonta, New York, effective September 1. Dr. Arnold was one of the founders of the Ohio Association for Adult Education in 1932 and has served as president of the Association since that time. He has served as president and secretary of the Psychological Section and as president of the Adult Education Section of the Ohio College Association.

Dr. Richard S. Solomon has resigned his position as instructor in psychology at Syracuse University to become staff director of the Personnel Institute, Inc., of Chicago. The Personnel Institute is a private organization which specializes in personnel control, job analyses, and vocational guidance.

The American Association for Applied Psychology will hold its annual professional conference December 1–3, 1939, in Washington, D. C. The Association had voted to meet with the International Congress of Psychotechnology, should it be convened in America, but that has proved impracticable. The by-laws of the Association provide that " insofar as possible the Association shall coördinate its program with that of the A. P. A." It did not seem the part of wisdom, however, to take the annual meeting of so young an Association so far from its center of population as would have been necessary if it had met this year with the A. P. A. The Association voted, therefore, to meet this year in the East at a time not in conflict with the A. P. A. meeting. There is no implication that the 2 Associations shall continue to meet separately.

The Washington-Baltimore branch of the American Psychological Association held its final meeting of the year at the University of Maryland, College Park, Maryland, on May 12. The following program was presented:

Dr. R. M. Bellows, University of Maryland: " Evaluation of Methods for Selecting Dental Students."
Mr. L. S. Guest, University of Maryland: " Study of the Immediate Recall of Radio Advertising."
Dr. John Finan, St. John's College: " Strength of Conditioning Under Varying Degrees of Hunger in Rats."
Dr. Karl M. Dallenbach, Cornell University: " Movies of American Psychologists."

The following officers were elected for the coming year:
President: Dr. Sidney M. Newhall, Johns Hopkins University.
Vice-President: Dr. Steuart Henderson Britt, George Washington University.
Secretary: Dr. Edwin Ghiselli, University of Maryland.
Treasurer: Mrs. Mildred St. M. Percy, District of Columbia Public Schools.

Professor Gemelli reports that in the National Board of Researches, a State organization which patronizes and supports scientific researches in any field, a Permanent Commission for the Application of Psychology has been recently appointed. The aim of this Commission is to promote among State institutions, labor organizations, and private persons the application of psychology in any field, with regard to selection and orientation as well as to the organization of work. At present, applications in the scholastic field, where professional orientation has become a function of the school, are being studied; applications in industries, aviation, and public transport will follow. The Commission is presided over by Professor P. Agostino Gemelli and includes both psychologists, among whom are Ponzo, Banissoni, Ferrari, Jr., Marzi, Galli, Colluci, and Galdo, and officials of State institutions, which purpose to use such applications of psychology.

Philosophic Abstracts, a quarterly review of philosophical books and periodicals in the form of brief excerpts and synopses, will start publication sometime in October. The purpose of this journal "is not only to present English-speaking philosophy departments and libraries with a bibliography of essential philosophic literature, but also to give them an opportunity to keep abreast with the principal philosophic theories as presented by its respective authors." Communications concerning this new journal should be addressed to 884 Riverside Drive, New York City.

The Symposium on Visual Fatigue, organized by the Committee on Visual Fatigue, under the chairmanship of Walter R. Miles, was held in the rooms of the National Research Council, 2101 Constitution Avenue, Washington, D. C., on May 20 and 21, 1939. The following program was presented:

Walter R. Miles, Laboratory of Physiological Psychology, Yale University School of Medicine: Introductory—"The Visual Fatigue Problem."

George Wald, Biological Laboratories, Harvard University: "The Chemical Basis of Visual Adaptation."

Clarence H. Graham, Psychology Laboratory, Brown University: "Frequency of Nerve Impulse Discharge as a Function of Time After Onset of Illumination."

Selig Hecht, Laboratory of Biophysics, Columbia University: "Relation Between Visual Acuity and Illumination."

Brian O'Brien, Institute of Optics, University of Rochester: "Iris Measurements."

P. G. Nutting, Jr., Research Laboratories, Eastman Kodak Company: "The Influence of Flicker Fatigue on Flicker Frequency."

Alfred Bielschowsky, The Dartmouth Eye Institute, Dartmouth Medical School: "Influence of Fatigue on the Mechanism Involved in Binocular Coöperation."

WALTER R. MILES, Laboratory of Physiological Psychology, Yale University School of Medicine: "Variations in the Polarity-Potential of the Human Eye."

FRANK K. MOSS, Lighting Research Laboratory, General Electric Company: "Visibility and Ease of Seeing."

ROSS A. MCFARLAND, Fatigue Laboratory, Harvard University: "The Effects of Anoxia on Certain Visual Functions."

MILES A. TINKER, Psychology Laboratory, University of Minnesota: "Visual Fatigue in the Reading of Print."

WALTER F. DEARBORN, Psycho-Educational Clinic, Harvard University: "On the Relations of Visual Fatigue to Reading Disability."

ROBERT K. LAMBERT, Eye Institute, Columbia University: "The Spasmogenic Tendency and Its Effect on the Eyes."

HARRY M. JOHNSON, Department of Psychology, Tulane University: "Rival Notions of the Nature of Physiological Impairment."

At the Round Table on Visual Fatigue, held on May 21, the speakers of the previous day endeavored to formulate research plans and projects in the general field of visual fatigue studies.

This 2-day conference was held at the suggestion of the Committee on Scientific Aids to Learning of the National Research Council.

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Psychological Bulletin

THE MEASUREMENT OF PERSISTENCE: AN HISTORICAL REVIEW

BY DAVID G. RYANS
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Though the significance of "persistence" was recognized early by the educator and the business executive, psychologists engaged in research gained interest but slowly in its experimental investigation. For many years the continuation of directed response was regarded simply as the functioning of "will," and treatment of related problems was accomplished philosophically or theologically. More recently, the prevalent practice in the ranks of psychology has been to consider persistent behavior as individually variable, specific to particular situations, and dependent upon organic drive, or, in human subjects, interest operating at a given time and providing the "set" for further activity. As such, persistence is looked upon as a subtopic under the more generic one of "motivation." Because of this assumed specificity of persistent behavior relatively few attempts have been made to analyze the trait or to reduce it to measurement.

This does not mean, however, that persistence has gone entirely unnoticed. Fernald, in 1912, wrote that ". . . the success or failure of individuals depends largely on the ability to endure and to continue to strive for the sake of achievement, in spite of fatigue and discouragement" (14, p. 331). Supporting this pronouncement, an experiment was outlined in which persons were graded according to their ability to "endure." A device was employed which required that subjects stand on their toes as long as possible, the ringing of a bell when their heels touched the base serving to announce their submission to the felt fatigue. Twelve nondelinquents tested by Fernald endured twice as long, on the average, as did 116 delinquents.

That these results were not fortuitous was indicated by a repetition of Fernald's approach and the introduction of an additional test involving the holding of 2-pound dumbbells at shoulder level, by

Bronner (3). The latter found very little overlapping between 2 groups tested, only a little over 11% of delinquent girls exceeding the median of college girls. "It would seem," Bronner comments, "that the members of Group C (college) were much more willing to endure physical discomfort for the sake of a good record than were members of Group D (delinquent). . . They (the members of Group D) seemed on the whole to have much less will power and physical endurance, at least in matters where there was no necessity for continued discomfort other than mere pride in a deed well accomplished" (p. 92). Both Fernald's and Bronner's studies may be considered investigations of persistent behavior.

Following Fernald and Bronner by several years, Haggerty (17), in an examination of trait ratings on successful men, noted and pointed out that successful individuals were characteristically persistent and industrious; and Stone (45), considering the disparity between intelligence and scholarship, proposed that the observable calculable relationship between those variables was low because, among other reasons, the factor of perseverance in the subjects was uncontrolled and varying.

SUBJECTIVE JUDGMENTS OF PERSISTENCE

Probably man has passed judgment upon the behavior of his fellows from the beginning of his existence. Such judgments, or ratings, involving nearly every human trait, have been based almost exclusively on one or the other of 2 approaches. Occasionally they have resulted from "behavior sampling," or observation of the subject in some particular situation which demands a response of the nature of that with respect to which the estimate is being made. These estimates often approach validity. More frequently, however, judgments of behavior have been arrived at, not by direct sampling, but through anatomical classification or through behavior observations of remotely associated traits. While it is assumed that these are related to the behavior in question, they actually may show complete lack of relationship. Such judgments are, for the most part, meaningless.

Persistence is often popularly judged after the latter manner. For example, set of jaw and facial angle have sometimes been associated with perseverance. Correlational results do not support such contentions. Cleeton and Knight (7) obtained ratings on "will power" from presumably capable judges and compared these with certain physiognomic measurements. The correlation coefficients for physical factors and "will" ratings were found to be -0.07 and

−0.04, respectively, when the ratings were made by "close associates" and by "casual observers." Similar results are reported by Sheldon (**41**), who set up correlation tables for morphological measurements compared with ratings on perseverance (defined as *tenacity* and *determination*). The average correlation coefficient was −0.02, suggesting absence of relationship between body-build and ratings on persistence.

Numerous attempts have been made to discover a link between handwriting and personality and character traits. The most classic of these is represented by the Downey Will-Temperament Test, which will be referred to presently. Results in this field, as in the case of anatomical judgments, have been consistently negative. Various claims made by the graphologists have associated persistence with such characteristics of handwriting as width of the downstrokes, connected letters, and the length of the crossbars on "t's." Miss L. E. Brown, working under the direction of Hull, found ratings on persistence to correlate −0.05 with width of the downstrokes and −0.03 with disconnected writing (**21**). Hull and Montgomery (**22**) report a correlation coefficient of 0.00 between ratings on perseverance and length of the "t" bars. (When different sizes of writing were compensated for, the r was 0.16.)

Downey's effort to measure "will-temperament" through the medium of handwriting was elaborate and extensive. The "will-profile" was introduced by Downey about 1919 (**11**) and more widely in 1923 (**12**). Twelve tests, based upon handwriting for the most part, comprised the battery. These tests[1] were scored separately and the results recorded graphically on a profile chart. From the standpoint of our discussion of persistence, it may be interesting to observe the nature of several of the tests. At least 5 may be thought of as relating to persistent behavior: *motor impulsion,* in which the subject was required to write his name under the influence of various distractions; *reaction to contradiction,* which purported to determine the readiness with which the subject would "give in" when contradicted; *resistance to opposition,* in which the subject was scored qualitatively as pressure was exerted against his pen point; *motor inhibition,* which required the greatest possible slowness of writing; and *volitional perseveration,* in which the subject was instructed to disguise his writing and was graded according to the time that he was willing to spend in practice.

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[1] Speed of movement, freedom from load, flexibility, speed of decision, motor impulsion, reaction to contradiction, resistance to opposition, finality of judgment, motor inhibition, interest in detail, coördination of impulses, and volitional perseveration.

Criticism, from the standpoints of both reliability and validity, falls hard on the Downey test. Reliability coefficients reported by Downey and Uhrbrock (**13**) and based upon repetitions of the tests after intervals of 1 day and 1 month range all the way from 0.09 to 0.89. The average of the coefficients seems to be about 0.60 according to these investigators. Reliability of the volitional perseveration[2] test, as measured by test-retest correlations, ranges from 0.25 to 0.58. It is decidedly low. Kennedy (**23**) also recently calculated test-retest correlations with the Downey Will-Temperament battery, the 2 administrations being 1 year apart. The coefficients ranged between 0.04 and 0.16. The reliability index for volitional perseveration was 0.07.

Considered, again, with respect to validity, the Downey tests fare no better. Ruch (**32**) presents correlations between ratings on the traits supposedly measured by the tests and actual test results. The coefficients vary considerably, all tending to be of a low order and some even negative. Symonds (**46**) combines 5 different sets of correlations between scores on the Downey test and ratings on the same traits. The average coefficients for motor impulsion, reaction to contradiction, resistance to opposition, motor inhibition, and volitional perseveration are 0.28, 0.14, 0.18, –0.02, and –0.03, respectively.

We may conclude that the Downey Will-Temperament Test, while it may have been a noble experiment, is not a suitable measuring instrument for traits of persistence.

Beginning with the studies of Webb in 1915, and continuing to the present time, extensive use has been made of *rating scales* in estimating persistence and perseverance. Webb (**52**) employed them to obtain the basic measurements for correlation in his statistical analysis and isolation of the "w" factor. Webb thought of "w," or "*will,*" as being made up of several component traits, chiefly *reliability, tact,* and *persistence of motives. "Persistence of motives,"* Webb explains, "may be understood to mean *consistency of action resulting from deliberate volition, or will*" (p. 60). The qualities which are said to characterize "w" are the same as those ordinarily popularly ascribed to persistence: (1) the tendency not to abandon tasks for mere changeability; and (2) the tendency not to abandon tasks in the face of obstacles. Evidence was accumulated by Webb to indicate that " w " represented a statistically independent trait of behavior.

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[2] The volitional perseveration test is that most frequently associated with "persistence."

Lankes (**25**) made use of persistence ratings in the comparison of the factors " p "[3] *(perseveration)* and " w." Likewise, the investigations of Haggerty, Cleeton and Knight, Hull, and Ruch (already referred to) were dependent upon rating techniques for indices of persistence. Rich (**30**) correlated certain physiological factors with persistence ratings, obtaining coefficients of low positive order between such estimates and the amount of acid and phosphorus in the urine of subjects. Flemming (**15**) used ratings of perseverance in connection with his analysis of achievement in high school. He found persistence, thus measured, to be related to the extent indicated by correlation coefficients of 0.72 and 0.35, respectively, with junior and senior high school populations. Similar results are reported by Hughes (**20**), who observed that honor students rank among the highest 25% as rated on persistence. Turney (**49**) applied a rating scale in determining the effect of persistence on success in a select group of high school pupils. He concluded that industry and perseverance were the 2 traits which best differentiated between achievers and nonachievers.

Many other studies of greater or lesser importance might be cited. No attempt will be made to survey all of the experiments which have included persistence ratings among their procedures. For the most part, the ratings have been incidental to some other feature. These ratings have varied considerably in their usefulness and applicability. Self-ratings generally have not been found to be highly valid. And ratings made by judges who are not well acquainted with the subjects, and by those who have had little experience in rating, lack reliability. But in the absence of objective tests *ratings,* properly prepared, *are highly acceptable substitutes.*

THE TESTING OF PERSISTENCE

The attempts to *test* persistence have been relatively few. This has not been due entirely to lethargy or to preoccupation on the part of the researchers, for there has been serious doubt in the minds of many as to the possibility of actually *measuring* the trait objectively. Morgan and Hull (**26**), for example, express their belief that experimenters are on the wrong track in trying to measure persistence in terms of quantity of work produced per unit of time. They insist that persistence cannot "be measured in the traditional manner" (p. 187). Rather, it must be determined through *qualitative judgments* of performance. The efficacy of the qualitative method is borne out, the authors believe, by results which they obtained in a well-planned experiment.

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[3] The discussion of " perseveration " and its measurement are not included in this review in so far as perseveration and persistence do not appear to be related significantly. See Ryans (**34**).

In the Morgan and Hull study 3 qualified judges rated 100 students on a scale of persistence consisting of 9 qualitative grades. The highest 20 and the lowest 20 subjects on this ranking were selected for further study and were presented with a maze situation which they were required to solve. While the subjects attempted to negotiate the maze they were observed by *experienced judges, who were unacquainted with the subjects.* These judges then rated the students on "persistence," the ratings being based exclusively upon performances in the immediate situations. Morgan and Hull report that ratings thus obtained are highly valid and that similar qualitative methods are the only ones that can be applied to persistence approximation.

Vernon (**50**) likewise holds that qualitative estimates based upon observation of behavior in specific situations should be employed in the study of traits of temperament and personality. In a recent publication he writes: "Personality and temperament are organized and integrated wholes which can not be put into simple quantitative terms; their estimates must be supplemented by direct qualitative observation and intuition" (p. 116). In applying such an approach Vernon observes that *persistence* (among such other traits as "aesthetic and constructive interests," "planning capacity," "emotional instability and variability," "introversion-extraversion," and "suggestibility") can be judged successfully by noting the remarks and behavior of subjects while engaged in simple performance tests and games.

Terman (**47**), writing in *Science,* expresses a view somewhat similar to the above-named authors.

But there *have* been some investigators in psychology who have not held the opinions of Morgan and Hull, Vernon, and Terman regarding the susceptibility of persistence to measurement. There are, in the literature, records of several notable assaults upon the problem of persistence in which the quantitative approach has been employed. These endeavors have not been thorough and complete in all cases, but they represent efforts to penetrate a relatively undisturbed field. Several of the more important studies of persistence will be mentioned. These are not all related: some deal with one phase of persistent behavior, some another; some employ as subjects preschool children, some adults; various relationships are reported. The present review will be guided simply by the chronological order of appearance of the reports.

What appears to be the earliest attempt to measure persistent behavior was that of Fernald, already described. Both Fernald and Bronner, measuring endurance, seem to have struck very close to the heart of the problem. How far understanding of the field would have advanced if the clues had been taken up and developed, it is difficult to say. Instead, however, the reports became bequests to the literature, and no extension of the methods was undertaken for many years.

Eleven years after Fernald's publication a short article by H. E. Burtt (4) appeared in *School & Society* under the title: " Measuring interest objectively." Burtt was concerned in this report with the comparison of such factors as *ability, interest,* and *industry* (or persistence). Industry was tested through the medium of prose passages, interspersed with irrelevant materials, which were to be read by the subjects. The subjects were required to cross out the irrelevant words as rapidly as possible, the author's assumption being that the more industrious individuals would tend to become so engrossed in the substance of their reading selections that they would overlook a greater number of irrelevant words than would their less industrious fellows. Upon examination of his data Burtt found that the number of words crossed out correlated —0.30 with instructors' estimates of interest and —0.20 with similar ratings on ability, thus inviting the conclusion that industry is positively, but lowly, related to the factors named. The results themselves hold no particular interest, but the approach was, to say the least, unique, and it offered suggestions that were utilized in later investigations of persistence.

Another early attempt to measure persistence objectively was Chapman's study of " word building " (5), published in 1924. Chapman, after considering the use of puzzles, riddles, and similar problems for his analysis, decided upon the formation of words from given letters as a measure of persistence because of such advantages as the familiarity of the general idea, the interesting nature of the task, its suitability for group use, and the objectivity of administration and scoring. Using as his subjects the pupils of eighth-grade classes, Chapman asked them to construct as many words as possible from a set of letters with which they were provided. Three aspects of individual performance were observed in their interrelationships: (1) *success* (total number of words obtained before cessation of work); (2) *speed of thought* (average time taken per word, up to the 10th word); and (3) *persistence* (the total time between the starting signal and individual admission that the limit had been reached). The study as a whole is both interesting and suggestive. Speed of thought, as measured, was found to correlate with persistence -0.11 to -0.44 (average $r=-0.29$), while success at word building correlated 0.48 to 0.81 (average $r=0.65$) with persistence time. An absence of apparent sex differences in the traits measured was reported.

Also in 1924 Blume's account (2) of the development of "will power" in school children was published. Persistence, or will power, was tested by requiring the children to gaze steadily at an object without blinking the eyelids. Studied genetically by this method, "will power" was found to improve with age between the ages tested (6 to 14 years).

With younger subjects and additional situations another investigator, Beyrl (1), reported like results several years later. Observing subjects of from 2 to 6 years of age in activities including (1) block sorting, (2) block building, (3) picture puzzle completion, (4) opening of puzzle box, and (5) playing with a toy "mouse and trap" puzzle, Beyrl concluded that time spent at such behavior was proportionately longer for the older children.

At about the same time the above-listed studies were in progress, a problem closely related to that of persistence (though not so-called) was being approached through certain tests devised originally by Moore and Gilliland. These were termed "aggressiveness tests." The revised battery consisted of tests (scored on a time basis) of eye-movement distraction, staring (similar to Blume's test), normal speed of handwriting, and word association. Gilliland (16) tells of the administration of these tests to liberal arts students, students enrolled in public-speaking courses, and salesmen. Intelligence seemed to be unrelated to aggressiveness (r's$=0.008$ and 0.027). Correlations of 0.024 and 0.34 between scholarship and aggressiveness were obtained for the liberal arts and public-speaking groups, respectively. Ratings on "selling ability" and aggressiveness test results correlated 0.26.

Undoubtedly the most extensive survey of persistence traits heretofore attempted was that in connection with the Character Education Inquiry. The report is made by Hartshorne, May, and Maller (18) in the second volume of *Studies in the nature of character* under the general heading of *Studies in service and self-*

control. The book deals with the estimation of, and relationships entered into by, persistence and inhibition. "Persistence" and "inhibition" are presumed to be demonstrations of self-control. They are not believed to be native or general traits of the individual, but rather, according to Hartshorne, May, and Maller, *result from learning* and, therefore, vary with specific situations. "Whether one persists in an act or refrains from making a suggested response, his action can be explained in terms of his previous experience unless he is under some form of external compulsion" (p. 279). Interest is the key to the functioning of persistence in all cases. ". . . we may think of both persistence and inhibition as functions of interest" (p. 280).

> "Capacity of one sort or another may be involved (*in the performance of an activity*). But it is involved through the medium of interest and not directly in the act itself. The individual differences in the actual situation are differences in the amount of interests. . . Absence of interest is absence of activity. Increased interest implies more prolonged activity. The interest may be in the satisfying quality of the activity itself apart from its more remote results . . . or may be in the end to be gained, even against the resistance of a disagreeable or painful activity required to achieve the desired ends" (p. 281; italics reviewer's).

The tests and results of the *Studies in service and self-control* are of interest in a review of persistence studies, especially from the standpoints of variety and completeness. A wide range of situations was covered. Part of the tests required performance; part were of the "pencil-and-paper" type. Some were administered individually; some were adapted for group use. It is pertinent to undertake a brief description of the instruments employed and to note the general results.

In all, some 8 tests of persistence were introduced in the study: (1) story resistance, (2) puzzle mastery, (3) paper-and-pencil puzzle solution, (4) fatigue and boredom in mental work, (5) hunting for hidden objects, (6) continued standing on right foot, (7) eating cracker and whistling, and (8) solving a toy puzzle.

In the story-resistance test the subject was given 2 printed stories, which were characterized by the increasing difficulty of reading (due to typography) as the plot progressed. The subject could continue to read to find out how the story ended, or he might begin a new story at any time. He was scored as he persisted in reading the first selection attempted. Puzzle mastery involved the solution of 1 of 2 mechanical puzzles with which the subject was presented. Either puzzle (Japanese Cross; Chinese Rings) might be chosen for solution. The scoring was based upon the time the subject spent before he " gave up " and turned to the second puzzle. The paper-and-pencil puzzles were of several varieties and were similar in administration and scoring to the mechanical puzzles. Fatigue or boredom was measured through the use of a series of addition problems. The difference between the number of problems worked during the last 4 minutes and the first 4 minutes of a 24-minute period constituted the score. (Two different incentives were applied in this part of the experiment; the subjects were measured on their disposition to work both for " self " and for the " class.")

The above tests were suitable for group use. The remaining situations required individual administration. The subjects were scored according to their willingness to exert themselves or to carry an activity through to efficient completion.

In that persistence and inhibition, as those traits are ordinarily thought of, appear to be very similar, considerable attention may well be given the latter in a discussion of persistent behavior. Indeed, the two seem to be reciprocal processes, and the operation of one assumes that of the other. The inhibition tests employed by Hartshorne, May, and Maller (**18**) consisted of 6 group tests: (1) story inhibition, (2) safe manipulation, (3) puzzle manipulation, (4) distraction, (5) picture inhibition, (6) candy inhibition; and 7 individual tests: (1) candy inhibition, (2) mirror and feather, (3) picture book—laughter, (4) bad odor resistance, (5) bad taste resistance, (6) resistance to startle stimuli, and (7) pain resistance. The inhibition tests required that some activity in progress, some well-established response, or some desired response be withheld during the course of the experimental period. The subject was scored according to his ability to inhibit his activity.

Numerous tables are presented by the authors of *Studies in service and self-control,* showing the reliabilities of various of the tests, their indices of validity, and internal and external relationships. On the whole, the tests do not appear to be highly reliable, the uncorrected coefficients of correlation ranging from below 0.40 to 0.85. Correction by application of the Spearman-Brown formula raises these correlations. Validity is also uniformly low as measured by comparison of the test results with teachers' ratings. The range of coefficients is from 0.00 to 0.33 for *persistence* tests and from 0.12 to 0.39 for *inhibition* tests with respect to which validity was calculated.

Correlation tables showing the interrelationships which existed between the specific tests employed in the study are given below. While incomplete, these serve to indicate the general trends.

TABLE I

COEFFICIENTS OF CORRELATION SHOWING INTERRELATIONSHIPS OF PERSISTENCE TESTS (GROUP) (**18**, p. 332)

	1	2	3	4	5
1. Story Resistance		0.35	0.16	0.15	0.18
2. Magic Squares			0.38	0.20	0.15
3. Cross and Ring				0.22	0.18
4. Addition: *Self*					0.42
5. Addition: *Class*					

Average $r=0.24$

TABLE II

COEFFICIENTS OF CORRELATION SHOWING INTERRELATIONSHIPS OF PERSISTENCE TESTS (INDIVIDUAL) (**18**, p. 340)

	1	2	3
1. Standing on One Foot		−0.11	0.10
2. Hunting for Hidden Objects			0.09
3. Work at Toy Puzzles			

TABLE III

COEFFICIENTS OF CORRELATION SHOWING INTERRELATIONSHIPS OF INHIBITION TESTS (GROUP) (**18**, p. 340)

	1	2	3	4	5	6
1. Story Inhibition		0.28	0.07	0.32	0.05	0.21
2. Manipulation of Toy Safe..			0.50	0.26	0.00	0.38
3. Puzzle Manipulation				0.31	0.18	0.33
4. Pictures					0.42	0.24
5. Ruggles Distraction Tests..						0.01
6. Candy Inhibition						

TABLE IV

COEFFICIENTS OF CORRELATION SHOWING INTERRELATIONSHIPS OF INHIBITION TESTS (INDIVIDUAL) (**18**, p. 351)

	1	2	3	4	5	6
1. Feather Resistance		−0.09	0.21	0.49	0.49	−0.14
2. Startle Resistance			0.17	−0.05	0.15	0.04
3. Taste Resistance				0.43	0.78	0.09
4. Odor Resistance					0.63	0.04
5. Humor Resistance						0.04
6. Candy Resistance						

Low positive correlations were reported between results of the persistence tests and intelligence test scores. There also seemed to be some tendency for persistence to increase with age, in so far as the ages considered (9 to 16 years) were concerned. Other findings growing out of the Character Education Inquiry included the following:

(1) There seemed to be little relation between persistence and grade status except that which could be accounted for by age.

(2) Persistence appeared to be unrelated to sex, though girls in the populations studied were slightly superior to boys.

(3) There was no apparent relation between persistence and physical condition.

(4) Siblings seemed to resemble one another in persistence.

(5) The less favored community (*of 3 studied*) and the foreign born in all communities seemed to be more persistent.

(6) Resistance to suggestion and persistence were positively and lowly related, the *r*'s being 0.12 to 0.28.

The Hartshorne, May, and Maller study of "self-control" represents an important contribution to the problem of persistence. It fell short, as have most other investigations of this nature, in that sufficient analysis of the tests employed was lacking. There was no serious attempt to establish the validity and suitability of the tests which were used.

Cushing's study (**9**), entitled "A perseverative tendency in preschool children: a study of personality differences," appeared during the same year, 1929, reporting the results of an investigation *(not of perseveration, but of persistence)* with young children as subjects. A number of situations were included in the project, scoring being accomplished by timing the individual as he continued in a line of activity. While the children varied noticeably in their demonstrations of persistent behavior, the intercorrelations between the tests were relatively high (average of intercorrelation coefficients =0.42) and seemed to suggest a "common factor" in operation.

Taking as an index of persistence the time spent working at the solution of a formboard, Nelson, in 1931, in an investigation of *Personality and intelligence* (**28**), obtained low positive correlation coefficients between persistence and mental age yielded by the Kuhlmann-Binet and Merrill-Palmer tests.

Wang (**51**), in 1932, proposed to measure persistence, not by testing in specific situations, but, rather, with a questionnaire devised somewhat after the same manner as the so-called "personality questionnaires" which have been used for measuring such traits as neurotic tendency, ascendance-submission, introversion-extraversion, etc. An original list of 181 questions was prepared by the author and distributed to 75 different judges for criticism and selection. After the questions had been rated with respect to their value as probable indicators of persistence, 101 items requiring "Yes" or "No" responses were administered to over 500 college students and the results studied. Reliability of the schedule as measured by correlating odd with even pages of items was represented by a coefficient of correlation of 0.84. Validity, determined by correlation with self-ratings of the subjects on persistence, was 0.52. Persistence scores of the subjects answering the schedule and their scores on the American Council of Education Intelligence Tests were slightly negatively related ($r = -0.05$). Persistence and "lack of neurotic tendency" (measured by the Thurstone Personality Schedule) correlated 0.51.

One recent approach to the study of persistence, that of Howells (19), has considered the problem almost wholly from the standpoint of *resistance to pain*. The experimental setup included 6 different situations, variously applied so as to render 11 estimates of persistence, or endurance. These situations (resistance to fatigue as determined by continued squeeze on a hand dynamometer, resistance to pain caused by pricking with a needle, resistance to heat from an electric grill applied to the body surface, resistance to electric shock, resistance to pinching of finger, and resistance to pain caused by forcing a blunt peg into the flesh) yielded intercorrelation coefficients ranging from 0.18 to 0.72.[4] Among others, the following external relationships were observed:

(1) a correlation of 0.81 between *persistence* and "volitional perseveration" as measured by the Downey Will-Temperament Test;

(2) a correlation of 0.44 between *persistence* and ascendance as measured by the Allport test;

(3) a correlation of 0.37 between *persistence* and religious radicalism as measured by ratings;

(4) a low positive correlation of 0.10 between *persistence* and intelligence test results;

(5) a multiple correlation of 0.64 between "intelligence-persistence" and grades made by university students;

(6) a tendency for the oldest children in a family to be more persistent;

(7) a slight tendency for the male to be more persistent than the female.

A comparison of some of the measures of persistence that had been studied was undertaken by Porter (29) and reported at the

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[4] The higher interrelationships were obtained when the results of the same test applied to different parts of the body were correlated, *i.e.* pain stimulus to the right hand and pain stimulus to the left hand.

1933 Chicago meeting of the American Psychological Association. Nine different methods were compared by administering the tests and scales to 410 college-student subjects and studying the results by correlational techniques. Fernald's volometer test, the dynamometer test (continued squeeze), the Morgan-Hull maze (qualitatively scored), a questionnaire (similar to that of Wang), a self-rating scale, a word-building test, a multiple choice test involving qualitative ratings, a work-limit examination, and an achievement index were studied. "The picture revealed . . . is one of almost complete lack of relationship among tests which have been considered valid tests of persistence. Two explanations of this lack of positive relationship present themselves: persistence is not a generalized trait, and at least some of the traits are not valid measures of persistence" (p. 644).

This is one of the few comparative analyses that have been made of persistence, and it is undoubtedly a valuable study. However, the reader cannot help regretting that the interrelationships were not further investigated in an effort to determine *which* of the measures were the more valid ones.

Landis (**24**), in an unpublished study of perseverance in the preschool child, used puzzle boxes in the testing situations. Slight relationship was found between the trait tested and chronological age, but a fairly close resemblance between perseverance and intelligence (*r* of 0.86 at the 5-year level) was revealed.[5] No significant sex differences were observable.

Four groups of London school children were tested by Crutcher (**8**) in an attempt to discover whether or not persistence was related to such factors as physical or mental defect, introversion, intelligence, and lack of family. Persistence was estimated by determining the individual time devoted to such tasks as card-house-building, mechanical puzzle solution, addition, picture copying, and cancelling "A's." Children of ages 7 to 16 served as subjects. Physical defectives, mental defectives, orphans, and normal children did not differ as groups in persistence. The number in each group was small. A correlation of 0.30 was found between persistence, as measured, and intelligence. The more persistent children seemed to be slightly inclined toward introversion. Available intercorrelations are given below.

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[5] The fact that Landis found such a high positive relationship between perseverance and intelligence, in the face of consistently low correlations reported by other investigators, suggests several questions: (1) that the intelligence test employed, the Minnesota Intelligence Test for the Preschool Child, may be more of a persistence test than an "intelligence" test; (2) that persistence and intelligence may become decreasingly related with advance of years; (3) that the puzzle-box situation may not be a valid test of persistence.

TABLE V

INTERCORRELATION COEFFICIENTS FOR 6 PERSISTENCE TESTS (8, p. 413)

	1	2	3	4	5	6
1. Card-house-building........		0.45	0.56	0.48	0.49	0.49
2. Mechanical Puzzle (A)....			0.71	0.37	0.29	0.41
3. Mechanical Puzzle (B)....				0.47	0.23	0.50
4. Addition...............					0.33	0.64
5. Picture Copying						0.30
6. Cancelling "A's"						

Interpreting the above table, Crutcher states that such factors as general intelligence, perseveration, determination, special abilities, and specific factors seem to play a part in determining degree of persistence possessed. He does *not* believe that there is a general trait of persistence. The intercorrelations suggest, however, that multiple factor analysis might reveal a tendency for the test results to be related through a general factor, which might be designated as *persistence*.

Dorcus (**10**) also has been engaged with the problem of "generality *vs.* specificity" of persistence, or *volitional perseveration.* An experimental investigation was reported in 1935 which attempted to determine if a "unit factor" of volitional perseveration (of the type described by Downey) existed. Four tests (continuous reaction, drawing a straight line as slowly as possible, solution of a mechanical puzzle, and solution of a cryptogram) were administered and correlated. Line drawing was negatively related to the other 3 tests. Continuous reaction, continued work at cryptogram, and continued work at the puzzle were positively correlated. Dorcus claimed to find little evidence, so far as the traits measured were concerned, of the existence of a general factor. Intelligence Quotients correlated −0.06, −0.05, 0.23, and 0.14, in order named, with continuous reaction, line drawing, cryptogram, and puzzle situations.

Reverting in method to Chapman's earlier work, Clark (**6**) recently employed word- and number-building in testing persistence. Clark experimented with a number of different tests, but eliminated all but 2 as unsatisfactory. The measures adopted, similar in nature, were (1) *word-building* and (2) *number-building*. *Word-building* required that as many words as possible be formed from a given set of letters. In *number-building,* 3 numbers were presented to the subject, who was instructed to seek a maximum of unique combinations. The validity of these measures, as indicated by correlation with teachers' ratings of "class-room perseverance" and "general perseverance" was relatively low. Coefficients of from 0.21 to 0.45 were obtained when the tests were administered during the school year, and *r*'s of from 0.49 to 0.76 were obtained when the tests were administered at the beginning of the term (at a time when the subjects were presumably more eager to make a good showing). Reliability, as it is possible to judge it from the above correlations, appears to have been low.

The writer has recently studied various aspects of the problem of persistent behavior and its measurement. Proposing that "persistence" represents one of the major concerns of psychology (**33**), as a science in its own right, the opinion is expressed that the trait may be described basically as "continued energy release in an adequate response of the organism" (**34**). It is held that the concept of persistence may be considered to be similar to that of "drive," as the term has been employed by Woodworth and Holt. In an experimental attempt to analyze persistent behavior, Ryans (**35**) selected for study 17 situations,[6] involving (a) continued effort in learning and problem-solving, (b) inhibition of reflexes and well-fixed habits, (c) endurance, or resistance to felt-fatigue and boredom, and (d) resistance to extraneous stimuli. It was presumed that such situations might demand *persistent* response on the part of a subject. Product moment correlation coefficients were computed for each measure with every other, and the resultant intercorrelations were subjected to multiple factor analysis, according to the "center of gravity" method described by Thurstone. A group factor emerged which was referred to as a *"persistence factor."* The table of factor loadings for the 17 items, with "intelligence" test performance added, is given on page 731.

In the light of the factor loadings some of the original measures appeared to be valid indicators of the trait, while others did not. After due consideration, 3 items were chosen to be employed in a battery (**36**). This battery attempted to measure persistent behavior through specific situations requiring (1) consecutive effort at rational learning (informational materials), (2) self-ratings on a "functional" schedule, and (3) physical endurance (continued arm extension). In its final form this test was administered to approximately 500 individuals of high school and junior college ages.

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[6] Quantitative estimates of individual behavior were obtained relating to code deciphering, anagrams (number), anagrams (time), study time (limited materials), inhibition of free-association responses, inhibition of wink reflex, inhibition of patellar reflex, continuous mental work (addition), physical endurance (arm extension), resistance to distraction, negative transfer or perseveration, study time (log), persistence ratings (paired comparisons), persistence ratings (graphic scale), honor-point ratio, a persistence schedule, and resistance to suggestion.

TABLE VI

FACTOR LOADINGS FOR ITEMS INCLUDED IN THE ANALYSIS OF PERSISTENCE MEASURES (**35**, p. 349)

	Factor		
Measure	I	II	III
1. Code Deciphering	.38	.19	—.14
2. Anagrams (number)	.52	.10	.13
3. Anagrams (time)	.38	—.33	.20
4. Study Time	.46	—.15	.35
5. Inhibited Free-Association	.26	—.07	.29
6. Inhibited Wink	—.30	—.18	.22
7. Inhibited Patellar Reflex	.39	.30	.22
8. Continuous Mental Work	.30	—.55	.10
9. Distraction Resistance	—.08	.13	.11
10. Physical Endurance	.30	—.31	—.13
11. Negative Transfer	.01	—.58	.26
12. Study Log	.53	.19	—.43
13. Intelligence Tests	.30	.69	.16
14. Persistence Rating (PC)	.89	.20	—.19
15. Persistence Rating (G)	.83	.32	.12
16. Honor-Point Ratio	.74	.30	—.15
17. Schedule	.29	—.03	—.71
18. Suggestion Resistance	.43	.39	.10

Ryans (**37**) reports the use of the persistence battery in a study of the relationship between persistence, intelligence, and academic success. In general, persistence, as measured, was found to be unrelated (or lowly positively related) to verbal ability as revealed by the Army Alpha Examination and the Otis Self-Administering Test. Persistence test performance seemed to be positively related to success in school, 2 samples yielding *r*'s of 0.48 and 0.38.

In a study of persistence scores compared with respect to the birthplace of the subject's male parent (**38**), tendencies were evident for first-generation Americans of Finnish, Norwegian, Serbian, and Austrian descent to surpass the average of the groups studied in persistent behavior, and for children of Croatians, Irish, and Slovenians to possess the capacity in considerably lesser amounts. The differences between groups were not statistically significant, the largest *Diff./S.D.(Diff.)* being 2.6.

Regarding variations in persistence score with sex, age, and academic level, Ryans (**40**) found that (1) outstanding sex differences were not apparent, though there was a slight tendency for the males to exceed the females in the cases observed; (2) within the age range tested (14 to 22) there was no consistent change in score from one level to another; and (3) in so far as junior college students attained a higher average score than did high school pupils, there was some evidence of educational selection for persistence.

When the scores of 40 junior college sophomores who took the test were studied with respect to certain "personality" traits as measured by the Bernreuter Inventory, negative correlation coefficients of a low order were obtained with all traits with the exception of "sociability" (**39**). Correlations and comparisons of extreme quarters of the persistence battery distribution indicated the possibility of a relationship existing between persistence, on the one hand, and "emotional stability," "extraversion," and "confidence in self," on the other.

Results reported by Thornton and Guilford (**48**) do not support Ryans' findings regarding the generality of a trait of persistence.[7] Their statistical analysis suggests not 1 factor, but at least 2, operative in persistent behavior. Thornton and Guilford obtained records for 189 university students with respect to 10 performance tests, a rating scale, a questionnaire, and 10 supplementary measures. Simple correlations were calculated and the results factor-analyzed.[8]

Of the 231 intercorrelations, only 58 were considered significant, these involving, for the most part, such tests purposing to relate to persistence as "time for holding breath," "time taken for word-building," "resistance to shock," "resistance to pressure," and the supplementary factors of "strength of grip" and "sex." As already stated, no evidence was found for the existence of a *single* factor universally present in the 22 items treated. Analysis did reveal 5

[7] That this discrepancy is at least partially a function of the diverse selection and grouping of tests and of the different methods of analysis employed is not improbable.

It may be further noted that opposed conclusions, drawn from the same original correlations, regarding the existence of general and group factors have been presented recently in the analysis of measures of intellectual traits (**44**).

[8] The items included in the factor analysis, as reported in a mimeographed table of intercorrelations, were: "time breath was held," "aiming time," "motor inhibition," "perceptual time," "perceptual amount," "word-building time," "number of words formed," "resistance to shock," "resistance to pressure," "maintained grip," Wang's schedule, persistence ratings, "verbal ability," "grip," "sex," "weight," "height," "recognition time," "reading speed," "word-building speed," "self-confidence rating," and "ascendance-submission rating."

common group factors. Two of these were tentatively described as (1) an ability and/or willingness to withstand discomfort in order to achieve a goal, and (2) a tendency to "keep on" at a task (plodding).[9] These seemed to resemble certain aspects of persistence. Additional factors were called "physical strength," "mental fluency," and "feeling of adequacy." With 2 exceptions, the performance tests of persistence employed yielded zero-order coefficients when correlated with measurements of "verbal ability" or "intelligence."

METHODOLOGY IN THE ESTIMATION OF PERSISTENCE

In so far as the concept of persistence, as employed both popularly and technically, has derived its meaning from that of its Latin cognate *persistere* (*per,* by way of; *sistere,* to stand) and conveys the idea of remaining unchanged or fixed in a line of action, it is not surprising that most investigators have sought to determine the existence of the trait through situations demanding continuation of an adequate and directed response.

The common approach has involved (1) a problem situation (stimulus) of presumably general effectiveness or response-value, (2) a relatively common response, or behavior pattern, the continuation of which might be expected to terminate in a solution or an attainment, and (3) interference with, or frustration of, the response before solution or attainment by fatigue, pain, boredom, or some other obstacle-factor. To date, little improvement of the basic method has been suggested. It is true, however, that variations of the technique probably have not been equally significant and that attempted analyses have not been of uniform value.

Ratings of persistence based upon general behavior observation, though frequently employed, are likely to be invalid not only because of ambiguity of definition of the trait, but also because of such ever-present sources of error as the "halo" and "central tendency" effects and the lack of qualifications on the parts of unselected judges. Furthermore, in self-ratings, obtained either through use of "questionnaire" or rating scale, the discrepancy between verbal response and performance enters into the picture to complicate conclusions. Needless to say, persistence ratings may provide useful and necessary supplementary data if care is observed in definition, if observation in uniform situations of sufficient breadth is required, and if trained and selected judges are employed. Methods such as those involved in the "paired comparisons" technique, though tedious, provide more adequate control over the rating process. Perhaps the most valid use of ratings with respect to perseverance was that of Morgan and Hull (**26**).

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[9] It appears probable that the analysis reported by Ryans (35) might have shown 2 common factors, comparable to those discovered by Thornton and Guilford, had additional measures of endurance or resistance to discomfort been included in the correlational matrix. Only 1, *arm extension,* was employed.

Numerous test situations, involving widely differing materials, have been set up in attempts to measure persistence. Unfortunately, many of these have been of doubtful reliability and validity, the common practice being to adopt measures which might, by inference, appear to be more or less directly related to persistent behavior. Frequently, no direct attempt at validation was made before the measures were employed for the purpose of drawing conclusions. Again, in one instance (**18**), measures were shown(?)[10] to be lacking in validity and reliability, yet were intercorrelated and the derived results used as bases for widely reported generalizations.

In the reviewer's opinion, the introduction of factor analysis methods has opened a promising avenue to the problem of persistence and the validation of measures of that trait. It must be recalled, however, that the results of the application of this statistical technique are influenced by many factors, including the number of variables employed, the pertinence of measures adopted, and the reliability of the measures. Properly determined, factor loadings should serve as validity coefficients and should enable the selection of valid measures of persistence.

Several controls which should guide subsequent work on, and classification of, the problems of persistent behavior are suggested by the studies which have been reviewed in this paper. They may be summarized in the following brief statements. First, serious attention should be given the matter of the validity of proposed tests of persistent behavior. Second, reliabilities of persistence measures should be determined whenever possible and reported. Third, recognizing that persistent behavior cannot exist in the relative absence of aptitude, capacity, or ability, attention should be directed toward this aspect of response in experiments, observations being made on subjects of comparable status and potentialities. Fourth, the interplay of "motivational" factors, along with aptitude and persistence, in determining behavior should be studied, and efforts extended in the direction of equalized motivation in persistence experiments. Fifth, care should be taken not to confuse persistent with perseverative behavior.

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[10] By correlating test results with ratings, the significance of which might be questioned.

IMPLICATIONS OF OBTAINED RESULTS

It is difficult to relate the results obtained in diversely planned and controlled studies of persistence to the findings and problems of neighboring areas of psychological interest. However, it may not be amiss to indicate certain implications, both as they might be judged to pertain at the present and as they may be revealed by later research. No effort will be made to expand such statements of relationship as are made.

Learning. The problem of learning, a basic one for psychology, is considerably complicated by (and is rendered incapable of adequate treatment without reference to) persistent behavior of the organism. Not only is this a matter of laboratory and theoretical interest, but it is one of extreme significance for education and for the understanding of the educational process.

Aptitude. Aptitude and capacity are likely to become confused with persistence in the total behavior pattern. This is readily understandable. To know more completely the relationship between aptitude and persistence is much desired. Although the possession of a minimum amount of capacity is necessary before persistent behavior is possible, it appears doubtful that, on the average, persistence and aptitude are closely related.

Interests and Motivation. Persistence, while not directly identifiable with interests, is probably closely related to these attitudinal responses, as well as to organic drives. Persistence and motivation appear to be complexly interwoven in behavior. That they are basically the same or different remains to be definitely determined.

Body Chemistry. Many psychological traits are being related to biochemical factors operating in the organism. Studies of the behavioral effects of the ductless glands suggest that persistent behavior may be greatly influenced. That incretions of the thyroid and the adrenal cortex, and possibly other of the endocrines, may be significant determiners of persistence does not appear unlikely.

Personality Configuration. Today, the clustering of some so-called personality traits and the apparent relative independence of others receive much attention. Persistence, as a contributing factor in the personality pattern, undoubtedly plays an important rôle. Its relation to such traits as ascendance, suggestibility, extraversion, radicalism, and emotional stability suggests widespread influence.

Social Behavior. Relationships, interesting to the sociologist and social psychologist, may exist between persistence and such group phenomena as competitiveness, social facilitation, and the like. They remain to be indicated.

Abnormality. Recent experiments producing neuroses through frustration of behavior suggest that persistence may be involved, and that light may be thrown on the psychiatric problem by a more thorough understanding of individual persistence.

SUMMARY

The determination of degree of persistence possessed by an individual has been attempted in diverse manners. *Qualitative ratings* have been regarded by some psychologists as the only adequate means of estimating persistent behavior. *Tests,* involving either (a) problem situations, (b) endurance, or (c) behavior presumed to be associated with, and indicative of, the trait, also have been employed under varying conditions and with dissimilar samples. Thirdly, *questionnaires* and schedules, like those which have been devised for the study of introversion and emotional stability have been used.

While the results of the several attacks on the problem of persistent behavior do not show great consistency, it may be stated tentatively that:

(1) intelligence, as measured by verbal tests, appears to be relatively unrelated, or lowly positively related, to measures of persistence;

(2) school marks seem to vary directly with measures of persistence;

(3) persistence, as tested, shows some evidence of increasing with age until adolescence; later age differences are not apparent;

(4) though males seem to show some superiority in recent tests, the sexes probably do not differ significantly in tendency to persist;

(5) national groups may differ, though insignificantly, in score on persistence measures;

(6) college students appear to manifest superior persistence traits as compared to high school pupils;

(7) nonpersistent individuals may show more pronounced leanings toward neuroticism and introversion than persistent persons;

(8) the extent to which an individual will endure fatigue, discomfort, or pain, the amount of time he will spend studying, and the amount of time he will spend working at specific tasks seem to be indicative of degree of persistence;

(9) resistance to suggestion, ascendance, and religious radicalism appear to be traits related to persistence;

(10) the existence of a general trait of persistence, which permeates all behavior of the organism, has not been established, though evidence both for and against such an assumption has been revealed.

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PROCEEDINGS OF THE FORTY-SEVENTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC., STANFORD UNIVERSITY AND THE UNIVERSITY OF CALIFORNIA, SEPTEMBER 4, 5, 6, 7, 1939

REPORT OF THE SECRETARY, WILLARD C. OLSON, UNIVERSITY OF MICHIGAN

The American Psychological Association, Inc., held its Forty-seventh Annual Meeting at Stanford University and the University of California. Sessions were held at Stanford University on September 4, 5, and 6, and meetings were then transferred to the campus of the University of California, at Berkeley, for September 7. A total of 514 persons registered, 140 being Members, 186 being Associate Members, 34 being newly elected Associates, and 154 being persons not affiliated with the Association. This was the first meeting to be held in the West, and, while the distance from the center of gravity of the membership was reflected in the registration, the attendance and interest was gratifying.

An analysis of the registration by geographical districts and states is as follows: New England States, 43 (Connecticut 18, Massachusetts 19, Rhode Island 2, New Hampshire 2, Maine 2, Vermont 0); Middle Atlantic States, 58 (New York 46, Pennsylvania 8, New Jersey 4, Delaware 0); South Atlantic States, 12 (District of Columbia 3, Maryland 2, Georgia 1, North Carolina 3, Florida 1, West Virginia 1, Virginia 1, South Carolina 0); East North Central States, 62 (Illinois 28, Indiana 1, Michigan 13, Ohio 13, Wisconsin 7); East South Central States, 12 (Kentucky 0, Tennessee 5, Mississippi 1, Louisiana 6, Alabama 0); West North Central States, 47 (Iowa 16, Kansas 4, Minnesota 14, Missouri 8, South Dakota 0, Nebraska 4, North Dakota 1); West South Central States, 11 (Arkansas 1, New Mexico 1, Texas 6, Oklahoma 3); Mountain States, 20 (Colorado 9, Wyoming 2, Arizona 2, Montana 2, Utah 4, Nevada 1); Pacific States, 245 (California 214, Oregon 12, Washington 16, Idaho 3); Canada, 1; England, 1; Hawaii, 2.

At the 1938 Business Meeting of the Association it was voted to distribute the *Psychological Bulletin* without added cost to the entire membership. This plan permitted the publication of the program in the July, 1939, issue of the *Psychological Bulletin* instead of the former plan of a preprint and subsequent printing in the November issue.

The program of 18 scientific sessions and 131 papers by Members and Associates was presented as scheduled with the following exceptions: Edward B. Greene acted as Chairman of the section on "Tests and Measurements" to permit Frank N. Freeman to attend an adjourned meeting of the Council. Percival M. Symonds acted as Chairman of the section on "Social Psychology" in the absence of Goodwin Watson.

Papers by the following contributors were withdrawn because of conditions over which they had no control: joint authors Harold P. Bergen and Joel Dean, Ernst Harms, George Katona, George Kreezer, O. H. Mowrer, L. J. O'Rourke, joint authors George Papanizolaou and John P. Seward, and Georgene H. Seward. In each instance the withdrawal was accompanied by an explanation of the circumstances.

The 11 research and instructional films were presented as scheduled on Monday evening, with the addition of a film entitled, "Electrical Potentials From the Human Eye," by Walter R. Miles, of Yale University.

The Presidential Address on the subject, "The Psychologist's Frame of Reference," was delivered by Gordon Allport in the Benjamin I. Wheeler Auditorium on the campus of the University of California on the afternoon of Thursday, September 7.

Members of the Department of Psychology of Stanford University and the University of California held an informal reception for members and guests of the American Psychological Association in the Lawrence Frost Amphitheatre at Stanford University on the afternoon of September 5 and in the University of California Union following the address of the President.

The Psychometric Society arranged a scientific session on "Psychometrics" and a round table on "The Meaning of Psychological Meaning in Mental Measurements" in collaboration with the Program Committee of the American Psychological Association. Ninety-nine persons registered for the meeting. The Business Meeting of the Society was held on Monday and was followed by a dinner meeting and the address of the President, Truman L. Kelley, on the subject, "The Future Psychology of Mental Factors." The following officers were elected: President: Karl J. Holzinger;

Secretary: Harold A. Edgerton; Treasurer: Albert K. Kurtz; Directors: Henry E. Garrett and Robert C. Tryon.

The Society for the Psychological Study of Social Issues arranged a session on "Social and Political Psychology" for Tuesday in collaboration with the Program Committee of the American Psychological Association. One hundred and thirty persons registered for the meeting. At the Annual Business Meeting on September 3 Edward C. Tolman, of the University of California, was elected Chairman for 1939–1940 and I. Krechevsky was continued as Secretary-Treasurer. Leonard W. Doob was elected as a new member of the Council for the term 1939–1940, and J. F. Brown, Hadley Cantril, Horace B. English, Kurt Lewin, T. C. Schneirla, and Ross Stagner were elected for the term 1939–1941. Following the business meeting George W. Hartmann delivered the Presidential Address on the subject, "Value as the Unifying Concept of the Social Sciences." The Society also sponsored round tables on the "Psychology of International Relations and the Prevention of War" and the "Psychology of Industrial Conflict."

Transactions of the Annual Business Meeting

Due notice having been given, the Annual Business Meeting of the American Psychological Association, Inc., a quorum being present, was held on September 5, 1939, in the Education Auditorium, Stanford University, California, at 8:15 P.M., with President Gordon Allport in the chair.

Upon motion duly made and seconded it was voted that the minutes of the Forty-sixth Annual Meeting at the Ohio State University be approved as printed in the November, 1938, issue of the *Psychological Bulletin*.

The Secretary distributed mimeographed materials covering Announcements and Recommendations of Council to serve as the agenda for the meeting.

On the recommendation of the Council of Directors it was voted to elect Karl Bühler, Raymond B. Cattell, Karl Duncker, and Kenneth S. Wagoner as Members of the Association.

On the recommendation of the Council of Directors it was voted to transfer the 53 Associates named below to the status of Member:

1. Benton, Arthur L.
2. Bousfield, Weston A.
3. Brogden, Wilfred J.
4. Bryngelson, Bryng
5. Cameron, Norman
6. Carpenter, Clarence R.
7. Clark, Brant
8. Cushing, Hazel M.
9. Dexter, Emily
10. Ford, Adelbert

11. Ghiselli, Edwin E.
12. Goodfellow, Louis D.
13. Greene, James E.
14. Halstead, Ward C.
15. Hanfmann, Eugenia
16. Hardy, Martha C.
17. Hebb, D. O.
18. Horton, George P.
19. Jenkins, William L.
20. Karwoski, Theodore
21. Klineberg, Otto
22. Lepley, William M.
23. Loucks, Roger B.
24. MacLeod, Robert B.
25. Maslow, A. H.
26. Meenes, Max
27. Meyer, George
28. Miller, Neal E.
29. Mitchell, Mildred B.
30. Newman, Edwin B.
31. Ojemann, Ralph H.
32. Patten, Everett F.
33. Peters, Henry N.
34. Piotrowski, Zygmunt
35. Portenier, Lillian
36. Price, Bronson
37. Richardson, Helen M.
38. Richardson, Marion W.
39. Rogers, Carl R.
40. Rulon, Philip J.
41. Scott, Thurman C.
42. Skeels, Harold M.
43. South, Earl B.
44. Spence, Kenneth W.
45. Stanton, Frank
46. Stott, Leland H.
47. Updegraff, Ruth
48. Vaughn, James
49. Wagner, Isabelle F.
50. Walton, William E.
51. Warren, Neil
52. Wedell, Carl H.
53. Winslow, Charles N.

On the recommendation of the Council of Directors it was voted to elect as Associates the 257 persons whose names appear below:

1. Abt, Lawrence E.
2. Adams, Lois Stark
3. Andrews, Mildred Elizabeth
4. Andrews, T. Gaylord
5. Angyal, Andras
6. Arnold, Dwight L.
7. Ash, Frederick Elton
8. Bader, Julia E.
9. Bailey, Donald Ward
10. Baldwin, Alfred L.
11. Bale, Elinor
12. Ball, Richard S.
13. Bardellini, Anna V.
14. Bartholomew, Pearl J.
15. Baxter, Edna Dorothy
16. Bayton, James Arthur
17. Bear, Robert Murray
18. Bechtel, Lyla Elvira
19. Bechtoldt, Harold Palmer
20. Benjamin, Harold R. W.
21. Bennett, Chester Clarke
22. Berdie, Ralph Freimuth
23. Beyers, Otto J.
24. Bihler, Rev. Hugh J.
25. Billingslea, Frederick Y.
26. Bishop, Ruth
27. Bittner, Reign H.
28. Blair, Glenn Myers
29. Blakey, Robert Irwin
30. Bolgar, Hedda
31. Bolton, Robert Lewis, Jr.
32. Bonniwell, Bernard Leonard
33. Brase, Herman H.
34. Brookhart, Elinor E.
35. Brown, Guy E., Jr.
36. Bruner, Jerome Seymour
37. Brunswik, Else Frenkel
38. Burack, Benjamin
39. Bures, Charles Edwin
40. Burnham, Robert Ward
41. Caldwell, Verne Vincent
42. Capwell, Dora Frances
43. Carey, Thomas F.
44. Carpenter, Mary C.
45. Carroll, James A.
46. Cartwright, Dorwin
47. Chapanis, Alphonse Robert E.
48. Chrisof, Cleo
49. Coakley, John D.
50. Colgan, Edward Joseph
51. Conkey, Ruth Clark
52. Cook, Walter Wellman
53. Coombs, Clyde Hamilton
54. Cotton, Carol Blanche
55. Cotzin, Milton
56. Cox, Kenneth
57. Criswell, Joan Henning
58. Curfman, Mary Martha
59. Curry, E. Thayer
60. Daniel, Robert Strongman
61. Deabler, Herdis LeRoy
62. Degenhardt, Elizabeth Tierney
63. Deisenroth, Carl Ellsworth
64. Dent, Earl Leroy
65. Dietz, Gertrude Ruth
66. Dods, Annette Elizabeth
67. DuBois, Donald Blynn
68. Dyer, Dorothy Tunell

69. Eccher, William Frank
70. Eckerson, Lois Dorothea
71. Eisaman, Lourene
72. Elliott, Frank Reel
73. Ellman, Lillian H.
74. Ellsworth, Ralph Eugene
75. Epstein, Leon Joseph
76. Ewart, Edwin
77. Failor, Leona Mae
78. Farber, Maurice Lee
79. Flannery, Jane
80. Fosberg, Irving Arthur
81. Foster, Katherine M.
82. Freeman, Max J.
83. French, John Robert Putnam, Jr.
84. French, Robert Linton
85. Friedrich, Jeannette Elizabeth
86. Fromm, Erika Oppenheimer
87. Gaberman, Joseph
88. Gage, Catharine Johnson
89. Gassert, M. Elizabeth
90. Geier, Frederic March
91. Geiger, H. E.
92. Gibbons, Charles C.
93. Giese, William James
94. Gittinger, John William
95. Goldberg, Meyer
96. Goodlett, Carlton Benjamin
97. Goodman, Howard W.
98. Grant, David A.
99. Guest, Lester Philip
100. Guetzkow, Harold
101. Haire, Mason
102. Hall, Robert Collier
103. Hayes, Elinor Guthrie
104. Hazelhurst, Jack Harris
105. Henderson, Robert William
106. Henry, Charles Eric
107. Herlihy, Charles Augustine
108. Holmes, John Louis
109. Holway, Alfred Harold
110. Hoskins, Albert Burleigh
111. Howes, Elizabeth Boyden
112. Imus, Henry Alfred
113. Irwin, Ralph Alexander
114. Jackson, Hazel Miriam
115. Jarrett, Rheem F.
116. Jeffress, Lloyd Alexander
117. Kadel, Marian Gardner
118. Katzoff, E. Taylor
119. Kerr, Jean Adelaide
120. Kirkpatrick, Forrest Hunter
121. Klebanoff, Seymour George
122. Kleemeier, Robert Watson
123. Klugman, Samuel F.
124. Knapp, Robert H.
125. Kramer, Lois Evelyn
126. Lacey, John Irving
127. Lacey, Oliver Lilburn
128. Lacklen, Robert Jones
129. Lane, George Gorham
130. Lawrence, Merle
131. Lawton, George
132. Levin, Bernard M.
133. Levine, Jacob
134. Levine, Kate Natalie
135. Lewis, William Drayton
136. Liberman, Alvin Meyer
137. Lindley, Clyde Joe
138. Lipman, Eli Allan
139. Lippitt, Ronald
140. Locke, Bernard
141. Loring, John Caleb Gould
142. Lumsdaine, Arthur Allen
143. Mack, Leonard John
144. MacLean, Charles Featherling
145. Madden, William Francis
146. Magaret, Gretchen Ann
147. Mann, Mary Bachman
148. Margolin, Sol Ellis
149. Martin, Richard F.
150. Marx, Lawrence Norbert
151. Matthews, Walter Mason
152. McClelland, David Clarence
153. McClintock, James Alfred
154. McGranahan, Donald Vincent
155. McNally, Lorraine L.
156. McNemar, Olga Williamson
157. Meister, Ralph Karl
158. Metzner, Charles Alfred
159. Meyer, Edith
160. Meyers, Charles Edward
161. Miller, Ray Starbuck
162. Minium, Edward Wheadon
163. Molish, Herman
164. Moncreiff, Ruth
165. Morel, Adele M.
166. Morrison, Garrett Weir
167. Neall, Harriet Elizabeth
168. Neff, William DuWayne
169. Nelson, Erland N. P.
170. Norden, Irmgard R.
171. Norman, Ralph David
172. Nyquist, Ewald B.
173. Obenchain, Irving Rock
174. Oden, Melita H.
175. Orlansky, Jesse
176. Ostrander, Jessie Marcia
177. Owens, William Abbott, Jr.
178. Page, Dorothy C.
179. Parks, Hannah Atwood
180. Payne, Robert Bryan
181. Phelan, Joseph G.
182. Philip, Brother Roger
183. Piziali, Perina
184. Prentice, William Courtney Hamilton
185. Prescott, Daniel Alfred
186. Rabin, Albert I.
187. Raimy, Victor Charles
188. Ralston, Alene
189. Ramsdell, Donald A.

190. Rasey, Marie I.
191. Reichenberg, Walburga
192. Rhine, J. B.
193. Robins, Margaret Erwin
194. Rose, Dorian Mabyl
195. Rotter, Julian Bernard
196. Rousseau, Blanche M.
197. Rowland, Loyd
198. Rubisoff, Rhea B.
199. Russell, David Harris
200. Salter, Mary Dinsmore
201. Saltzman, Sara
202. Sartain, Aaron Q.
203. Scheerer, Martin
204. Schlatter, M. Josephine
205. Searle, Lloyd Vernon
206. Senderling, Ellwood Wesley
207. Sheehan, Mary Rose
208. Shulman, Kermit R.
209. Silvey, Clel Thurman
210. Simpson, Lowell Vogel
211. Sisk, Henry L.
212. Sjaardema, Hendrikus
213. Sless, Bernard
214. Snyder, William Ulrich
215. Stam, Ira Webster
216. St. Clair, Walter Francis
217. Stephens, Georgiana Lee
218. Stone, C. Harold
219. Strother, George B.
220. Sunley, John H.
221. Swartz, Blair K.
222. Temple, William Jameson
223. Thompson, Charles Eugene
224. Thomsen, Arnold
225. Thorpe, Louis P.
226. Townsend, Rebecca Reynolds
227. Tresselt, Margaret Elizabeth
228. Tuttle, Harold Saxe
229. Tyler, Leona Elizabeth
230. Uphoff, Howard Franklin
231. Usry, Hazel Delle
232. Verplanck, William Samuel, Jr.
233. von Lauenstein, Otto
234. Voth, Albert C.
235. Walborn, Rebecca Shannon
236. Waldrop, Robert S.
237. Walker, Katherine Constance
238. Webb, Mildred Watson
239. Weisenfluh, Norman
240. Weisskopf, Edith A.
241. Westerhof, Anthony Cornelius
242. Whisler, Lawrence Dale
243. Whitten, Ida Edith
244. Wickens, Carol
245. Wickersham, Francis M.
246. Williamson, James C.
247. Winfrey, Mary Elnora
248. Witmer, Frances Joan
249. Wittenborn, John Richard
250. Wolf, Irvin Simon
251. Wolfe, Ronald Milton
252. Wolfson, William Quitman
253. Woodward, Patricia Lucile
254. Young, Albert Leon
255. Zabarenko, Ralph Neft
256. Zerga, Josef Edmund
257. Zilboorg, Gregory

The Secretary announced the death of the following Members and Associates: Edward Herbert Cameron, December 20, 1938; Thomas Russell Garth, April 20, 1939; Selma M. Mathews, June 4, 1938; William E. McClure, June 8, 1939; John Holman McFadden, May 28, 1939; John W. McGarvey, December 26, 1938; Elmer B. Royer, April 3, 1939; and Walter G. Summers, September 24, 1938.

The Secretary announced the resignation of the following 3 Members: C. Macfie Campbell, Lawrence W. Cole, and Llewellyn Truman Spencer.

The Secretary announced the resignation of the following 26 Associates: Eleanor S. Brussell, Margaret Miller Chenery, A. J. Foy Cross, Evelyn Y. Ehrman, J. C. Ellickson, Ilse Forest, Margaret Abigail Forrest, F. Alexander Goldman, Elaine Baldwin Graves, Marion F. Greenham Greer, Oswald Lionel Harvey, Max F. Hausmann, H. Birnet Hovey, Edward L. Kemp, Charles A. Knehr, Howard A. Lane, S. Geraldine Longwell, William Malamud, Marie Hackl Means, Walter Stephen Newell, Jesse Batley Rhinehart, Jessie

Brainard Southgate, Dorothy Triplett, E. Frances Wells, Ella L. Wieg, Cary C. Wood, and Horace G. Wyatt.

The Secretary announced that the following 3 Members have applied for and have received the status of Life Member: Charles B. Bliss, William H. Burnham, and Joseph Jastrow.

The Secretary announced that the Council of Directors unanimously approved the actions of the President in making the following appointments:

Dr. Buford J. Johnson, of Johns Hopkins University, to act as a representative of the American Psychological Association at the celebration of the fiftieth anniversary of the opening of Goucher College on October 14, 15, and 16, 1938.

Dr. Calvin S. Hall, of Western Reserve University, to act as a representative of the American Psychological Association at the inauguration of Charles Burgess Ketcham as President of Mount Union College on October 20, 1938.

Dr. H. L. Hollingworth, of Columbia University, to act as a representative of the American Psychological Association at the installation of Edwin Sharp Burdell as Director of the Cooper Union for the Advancement of Science and Art on November 3, 1938.

Dr. Walter F. Dearborn, of Harvard University, to act as a representative of the American Psychological Association at the inauguration of Leonard Carmichael as President of Tufts College on November 4, 1938.

Dr. Emily L. Stogdill, of Ohio State University, to act as a representative of the American Psychological Association at the annual meeting of the American Council of Guidance and Personnel Associations from February 21 through February 25, 1939.

Dr. Robert A. Brotemarkle, of the University of Pennsylvania, Dr. Daniel Katz, of Princeton University, and Dr. Morris S. Viteles, of the University of Pennsylvania, to act as representatives of the American Psychological Association at the forty-third annual meeting of the American Academy of Political and Social Science on March 31 and April 1, 1939.

Dr. Arthur Melville Jordan, of the University of North Carolina, to act as a representative of the American Psychological Association at the centennial celebration of Duke University on April 21, 22, and 23, 1939.

Dr. L. J. O'Rourke, of the United States Civil Service Commission, to act as a representative of the American Psychological Association at the sesquicentennial anniversary celebration of Georgetown College from May 28 to June 3, 1939.

Dr. Robert Brodie MacLeod, of Swarthmore College, to act as a representative of the American Psychological Association at the centennial celebration in honor of Théodule Ribot on June 20, 1939.

Dr. John G. Jenkins, of the University of Maryland, to act as a representative of the American Psychological Association at the semicentennial anniversary of the Catholic University of America on November 11, 12, and 13, 1939.

Dr. David Ballin Klein, of the University of Texas, to act as a representative of the American Psychological Association at the inauguration of Homer Price Rainey as President of the University of Texas on December 7, 8, and 9, 1939.

The Forty-eighth Annual Meeting of the American Psychological Association will be held on the campus of Pennsylvania State College on Wednesday, Thursday, Friday, and Saturday, September 4, 5, 6, and 7, 1940. Bruce V. Moore, of Pennsylvania State College, is the local member of the Executive Committee for 1939–1940.

The Secretary announced the interim appointment of Karl F. Muenzinger by Council to the Council of Directors of the American Psychological Association for the year 1938–1939 to finish the unexpired term of Gordon W. Allport, who was elected President of the Association.

The Secretary announced the appointment of Leonard Carmichael and Horace B. English to the Advisory Council on Human Relations, which was created by the American Association for the Advancement of Science.

The Secretary announced that the following persons were selected by the Chairman, Robert S. Woodworth, and approved by Council as members on the Committee on Scientific and Professional Ethics: W. V. Bingham, Edward A. Bott, Harvey A. Carr, Franklin Fearing, H. M. Johnson, Truman Lee Kelley, and Donald G. Marquis.

The Secretary announced the appointment by the President and Council of the following members to the Committee on Displaced Foreign Psychologists for the year 1938–1939: Barbara S. Burks, Chairman; Luton Ackerson, William E. Blatz, David Ballin Klein, Gardner Murphy, A. T. Poffenberger, George D. Stoddard, Edward Chace Tolman, and Max Wertheimer.

The Secretary announced the resignation of Ben D. Wood as Chairman of the Committee on the Preparation of Examination Questions.

The Secretary announced that the third joint meeting of the Council of Directors and Board of Editors was held on Tuesday, September 3, 1939, at which time reports on editorial and business policies were discussed.

The Secretary announced that Samuel W. Fernberger was reëlected as Editor of the *Journal of Experimental Psychology* by the Electoral Board as prescribed in the Constitution.

The Secretary announced that 807 copies of the reprint of the Report of the American Psychological Association's Committee on the Teaching of Psychology in Teachers' Colleges were sent to presidents and instructors in these institutions. A notice concerning requirements for membership in national societies in psychology was inserted with each reprint as recommended at the 1938 meeting.

The Secretary reported that the following resolution was passed at the 1939 Annual Meeting of the Inter-Society Color Council and forwarded to the American Psychological Association:

> "Resolved that we express to the American Psychological Association our cordial thanks and appreciation for the coöperation they have extended to the Inter-Society Color Council in co-sponsoring the technical session on color tolerances of the 1939 annual meeting."

The Council of Directors expressed their wish to thank David Wechsler for the draft of the greetings commemorating the centenary of the birth of Théodule Ribot.

The Council of Directors and Board of Editors announced that they have assumed the sponsorship of publication without financial obligation of "Reference List of Abstracts, a Supplement to the *Psychological Index,* 1919–1928, volumes 26–35." Sales will be directed from the Business Office with a commission of 10% to reimburse the Association.

The Secretary announced that the Twelfth International Congress of Psychology will be held in Edinburgh, Scotland, July 22–27, 1940.

The Council of Directors announced that the Business Manager of Publications was authorized at the joint meeting with the Board of Editors to offer to Members and Associates the publications of the Association at club rates.

At the 1938 meeting the Association voted that the Council of Directors be empowered to include the *Yearbook* as a regular or added number of the *Bulletin,* if, after investigation, it appeared desirable to do so. The problems and possible savings have been reviewed with the Secretary and the Board of Editors and a decision made that no immediate steps be taken to issue the *Yearbook* as a number of the *Psychological Bulletin.*

Upon recommendation of the Council of Directors the Secretary will compile the *Yearbook* so that the geographical and institutional directory will also serve to designate administrative officers in departments of psychology and various institutions.

The Election Committee, John F. Dashiell, Chairman, announced the election of the following officers by mail ballot:

President for 1939–1940: Leonard Carmichael, Tufts College.

Directors for 1939–1942: Elmer A. Culler, University of Rochester; and Horace B. English, Ohio State University.

Nominees for appointment to the Division of Anthropology and Psychology of the National Research Council: Elmer A. Culler, University of Rochester; Edwin R. Guthrie, University of Washington; and Wolfgang Köhler, Swarthmore College.

Representative on the Social Science Research Council: Mark A. May, Yale University.

Upon nomination of the Council of Directors, Edna Heidbreder was elected to Council for the unexpired term (1939–1940) of Leonard Carmichael, newly elected President.

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the Program Committee and ordered the report printed in the Proceedings. (See Reports.)

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the Committee on Precautions in Animal Experimentation and ordered the report printed in the Proceedings. (See Reports.)

On the recommendation of the Council of Directors the Association voted to appoint Norman R. F. Maier, of the University of Michigan, as a member of the Committee on Precautions in Animal Experimentation for the term 1939–1942, and to make Harry F. Harlow, senior member, Chairman for the year 1939–1940.

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the Committee on Motion Pictures and Sound Recording Devices in Instruction of Psychology and ordered the report printed in the Proceedings. (See Reports.) The Association further voted that the Committee be continued with Willard L. Valentine as the new Chairman and Forrest Lee Dimmick as an added member.

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the Committee on Psychology and the Public Service and ordered the report printed in the Proceedings. (See Reports.) The Association further voted that the Committee be continued with the addition of Kimball Young and Rensis Likert to the present membership.

On the recommendation of the Council of Directors the Association voted to adopt the following resolution protesting the curtailment of psychological and psychiatric service in penal institutions in New York State:

"Be it resolved that the American Psychological Association deplores the recent action of the New York State Legislature by which certain positions of psychologist and psychiatrist have been summarily abolished in the penal institutions of New York State. This action sets a precedent which other states may follow since New York has always been regarded as a pioneer in modern penological methods. The Association believes such a move will tend to increase the amount and cost of crime; that it is false economy and distinctly against public welfare.

"Be it further resolved that copies of this resolution be sent to Governor Lehman and to other appropriate state officials."

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the Committee on Press Relations, ordered the report printed in the Proceedings (see Reports), and the Committee continued. The Association further voted that the specific recommendations of the report be referred to the Program Committee for consideration.

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the Committee on Displaced Foreign Psychologists and ordered the report printed in the Proceedings. (See Reports.) The Association further voted that the following members constitute the Committee for 1939–1940: B. S. Burks, Chairman; Gordon Allport, W. A. Hunt, D. B. Klein, Gardner Murphy, Saul Rosenzweig, E. C. Tolman, and Max Wertheimer.

On the recommendation of the Council of Directors the Association voted to appoint a Committee on Personnel, Promotion, and Public Relations to consider the coördination of the services now rendered by the Committees on Public Service, on Press Relations, on Displaced Foreign Psychologists, and the Joint Committee of the American Psychological Association and the Psychologists' League; and to consider the development of related services that will promote the advancement of psychology as science through the opening of new fields of psychological activity in schools and elsewhere, the extension of psychological services in the interests of national welfare, and the effective placement of psychologists. The Association voted that this Committee be directed to report a plan for such reorganization and extension of services at the next Annual Meeting; and that in the meantime it render such aid as it can to the duly constituted Committees mentioned above. The Association further voted that this Committee be constituted of representatives of the above-named Committees now existing, and a Chairman to be appointed by the incoming President, and that such subcommittees be appointed by this group with the coöperation and approval of the Council as seems desirable. The Association voted that this Committee endeavor to secure the coöperation of the American Association for Applied Psychology and other societies affiliated with the American Psychological Association having similar interests and problems.

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the Joint Committee of the American Psychological Association and the Psychologists' League and that the Association's representatives be continued. It was further voted that the report submitted by the Joint Committee be referred to the newly created Committee on Personnel, Promotion, and Public Relations; and that the Association's representatives be instructed to consider the coördination of their work with that of the new Committee.

On the recommendation of the Council of Directors the Association voted to acknowledge with thanks the informal report of progress of the Committee on the Preparation of Examination Questions in Psychology and to continue the Committee with Alvin C. Eurich, of Stanford University, as the new Chairman.

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the Advisory Committee on the *Psychological Index,* ordered the report printed in the Proceedings, and the Committee continued with its present membership. (See Reports.)

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the Committee on Investments and ordered the report printed in the Proceedings. (See Reports.)

On the recommendation of the Council of Directors the Association voted to acknowledge with thanks the informal report of progress of the Committee on Scientific and Professional Ethics and to continue the Committee.

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of its representatives to the American Association for the Advancement of Science and ordered the report printed in the Proceedings. (See Reports.) It was further voted to continue Walter R. Miles, of Yale University, and John A. McGeoch, of the State University of Iowa, as representatives for 1940.

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the representative, H. B. English, to the Advisory Council on Human Relations of the American Association for the Advancement of Science, ordered the report printed in the Proceedings, and Professor English continued as representative.

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the representative to the American Documentation Institute, ordered the report printed in the Proceedings, and voted that John E. Anderson be continued as representative. (See Reports.)

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the delegates to the Inter-Society Color Council, ordered the report printed in the Proceedings, and voted continuance of affiliation with the Inter-Society Color Council for 1939–1940. (See Reports.) The Association further voted to reappoint the voting delegates and additional delegates to the Inter-Society Color Council for the term 1939–1940 and to appoint Louise L. Sloan to fill the vacancy on the delegation.

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the representative, Percival M. Symonds, to the National Occupational Conference and ordered the report to be printed in the Proceedings. The Association voted to name no new representative, since the organization will disband on September 30, 1939. (See Reports.)

On the recommendation of the Council of Directors the Association voted to accept with thanks the report by Leonard Carmichael on behalf of the Association's representatives on the National Research Council and ordered the report printed in the Proceedings. (See Reports.)

On the recommendation of the Council of Directors the Association voted to accept with thanks the report of the senior representative, Mark A. May, to the Social Science Research Council and ordered the report printed in the Proceedings. (See Reports.)

On the recommendation of the Council of Directors the Association voted to hold the Forty-ninth Annual Meeting of the Association at Northwestern University in September, 1941, and the Semicentennial Meeting at Harvard University in September, 1942, which is also the centennial of the birth of William James. The Association further voted that A. R. Gilliland be made a member of the Executive Committee for 1941 and that Gordon Allport be made a member of the Executive Committee for 1942.

On the recommendation of the Council of Directors the Association voted to construe the term of elective officers as beginning on the day following the close of the meetings at which their election is announced.

On the recommendation of the Council of Directors the Association voted to create a Committee on Calendar Reform to investigate the advantages and disadvantages of the proposals to psychologists and report recommendations to the Association at the 1940 meeting. The Association voted to appoint Horace B. English as Chairman, and E. B. Delabarre, of Brown University, as a member of the Committee.

On the recommendation of the Council of Directors the Association voted that the Secretary and Treasurer attempt to obtain a certificate from the United States Treasury Department indicating that contributions made to the Association may be deducted from the income tax of the donor.

On the recommendation of the Council of Directors the Association voted to authorize the Treasurer to receive gifts for special purposes within the scope of work of the Association and to pay such sums for the purposes designated by the donors, and that all gifts to the Association, its committees, or agents, and the disbursement of the same be thus recorded on the books of the Association.

On the recommendation of the Council of Directors the Association voted to approve the Treasurer's Report for 1938 and ordered it printed in the proceedings. (See Reports.)

On the recommendation of the Council of Directors the Association voted to approve the Treasurer's budget for 1940 and ordered it printed in the Proceedings. (See Reports.)

On the recommendation of the Council of Directors the Association voted to create a Committee on the Constitution to reconsider the by-laws pertaining to the qualifications and privileges of Associates and Members, the method of nominating Council members, and such other related matters as may be brought to their attention. This Committee is asked to report to Council at the 1940 meeting of the Association. The Association voted to appoint the following Members to the Committee: John F. Dashiell, Chairman; Luton Ackerson, John E. Anderson, S. W. Fernberger, Robert B. MacLeod, Karl F. Muenzinger, Bronson Price, Dael L. Wolfle, and R. S. Woodworth.

On the recommendation of the Council of Directors the Association voted to appoint E. A. Culler as Chairman of the Program Committee for the 1940 meeting, with Forrest Kingsbury and the Secretary as members.

On the recommendation of the Council of Directors the Association voted to adopt the following resolution:

> Be it resolved that the American Psychological Association inform the International Committee of the International Congress of Psychology that it hopes that plans can be developed to hold an International Congress in the United States at such time as a representative congress can be arranged. The American Psychological Association further expresses its willingness to do all in its power to thus assist in the rehabilitation of scientific world psychology.

On the recommendation of the Council of Directors the Association voted to adopt the following resolution:

> Be it resolved that this Association deems it essential to the advancement of psychology as a science that the conditions of appointment, tenure, and promotion of competent psychologists in their professional positions be not affected by considerations of race, nor by their political views, nor by their exercise of any constitutional rights of citizenship, so long as the exercise of these rights does not interfere with their professional and scholarly competence.
>
> Be it further resolved that inasmuch as evidence presented to the Council of this Association indicates that certain of these considerations did influence adversely the decision of the Regents of the University of Colorado respecting the terms of appointment of Dr. I. Krechevsky, this Association go on record as deploring any affront to academic freedom and to the progress of psychological science.
>
> Be it further resolved that a copy of this resolution be sent to the President and Regents of the University of Colorado and be printed in the Proceedings of this meeting.

On the recommendation of the Council of Directors the Association voted to adopt the following resolution:

> Be it resolved that the American Psychological Association, believing democratic institutions to be the indispensable foundation of free scientific inquiry into human behavior, record its conviction that earnest efforts should be made by American psychologists to study all those phases of American life and opinion which bear on the safeguarding of our fundamental liberties and peaceful way of life.
>
> Be it resolved, further, that this resolution be communicated to all Committees of the Association, to its Editorial Board, and to its representatives to Councils and to other organizations, with the instruction that in so far as the functions of these bodies are in any way concerned with the issue herein stated, efforts be made to implement this resolution with appropriate action.

Upon motion by Professor J. P. Guilford, duly seconded, the Association adopted the following resolution:

Be it resolved that the American Psychological Association, assembled at the Forty-seventh Annual Meeting, express its thanks to President Ray Lyman Wilbur of Stanford University and President Robert Gordon Sproul of the University of California; to Professors Stone and Brown in charge of local arrangements; and to psychologists and other officers, members, and graduate students of the 2 Universities for the high quality of the arrangements made for the meetings of the Association and the cordial hospitality enjoyed.

The meeting adjourned at 9:10 P.M.

REPORTS

REPORT OF THE PROGRAM COMMITTEE OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

To the Council of Directors and Members of the American Psychological Association:

The "Call for Papers," after due revision, was issued by the Program Committee at the usual time; in reply, 131 abstracts were received directly from the membership. This figure is appreciably lower than the corresponding number (213) for 1938, the reduction being presumably due to distance of the place of meeting from the Association's center of population. Since the total number of submissions fell within the range of recent programs, every effort was made to schedule all papers that met the technical requirements. While this latitude of acceptance obviously cannot be continued in years when the number of abstracts far exceeds the capacity of the program, your Committee believes that, as a matter of policy, every effort should be made to admit all papers from both Members and Associates. Membership in the Association should constitute evidence *prima facie* that the abstract is worthy of appearing in the Program. The privilege of addressing the Association in its Annual Meeting is not a favor to be granted or withheld by the Program Committee; it is a right conferred by the Association itself in the act of election. This right is subject only to the equal rights of other Members and to the practical requirement of keeping the overall length of sessions within acceptable limits. The 131 abstracts above noted were distributed in 18 sections, 16 of which were scheduled for Stanford University on September 4, 5, and 6, and 2 for the University of California on the morning of September 7.

The current Program differs from those of former years primarily in the Program Schedule which precedes the abstracts. Now that a segment of time (15 to 20 minutes) is reserved for each paper in a session, the Committee believed that our members would appreciate a condensed time table, listing on a single page all concurrent programs and the temporal distribution of the component papers. The schedule as now amplified is designed to provide this information in convenient form. Inasmuch as this innovation slightly increases the cost of producing the Program, we suggest that the Program Committee for next year poll our members about the wisdom of retaining this feature. The Secretary will welcome expression of opinion, pro and contra, by the membership.

In addition to the 131 abstracts above mentioned, the Committee admitted to the Program 6 papers on Psychometrics, which were approved by the Program Committee of the Psychometric Society, and 5 papers on Social and Political Psychology, which were submitted by the Program Committee of the Society for the Psychological Study of Social Issues. One hundred and forty-two abstracts in 20 sections were thus scheduled for the Program as compared to 178 in 1938. Eleven films were included in a program on Research and Instructional Films for Monday evening, September 4. Upon recommendation of the Executive Committee, the Program Committee departed from our long-established practice of setting the Presidential Address for the final evening of the meetings. It was scheduled, instead, for the afternoon of September 7 at the University of California.

A round table on "The Meaning of Psychological Meaning in Mental Measurement" was arranged at the request of the Psychometric Society, and round tables on "The Psychology of International Relations and the Prevention of War" and "The Psychology of Industrial Conflict" at the request of the Society for the Psychological Study of Social Issues. Round tables on the "Elementary Course in Psychology" and "The Relation of Conditioning to Verbal and Voluntary Behavior" were arranged at the request of Members of the American Psychological Association.

The Committee voted to instruct the Secretary to include in all letters of acceptance a statement suggesting the desirability of sending complete copies of papers to Professor Stone at Stanford University for the use of press representatives. The Committee also urges that section Chairmen adhere closely to the printed time-schedule. In conformity with the action of the Association, at the 1938 Meeting, the present Program was printed as one issue of the *Psychological Bulletin*, dated July, 1939.

Respectfully submitted,

EDWARD B. GREENE
WILLARD C. OLSON
ELMER CULLER, *Chairman*

REPORT OF THE COMMITTEE ON PRECAUTIONS IN ANIMAL EXPERIMENTATION

To the Council of Directors and Members of the American Psychological Association:

Your Committee has continued to enjoy the coöperation of the Bureau of Legal Medicine and legislation of the American Medical Association during the past year. Since no legislation tending further to restrict legitimate experimentation upon animals appears to have been offered during the past year and since no infraction by a member of the rules of the Association has been reported, the Committee's activities during the year have consisted solely in circulating the "Rules Regarding Animals" in response to requests from laboratories engaging in animal experimentation. It was necessary during the year to have these rules reprinted and they continue to be available upon application to the Committee or to the Secretary of the Association.

Respectfully submitted,

HARRY F. HARLOW
K. F. MUENZINGER
DONALD K. ADAMS, *Chairman*

REPORT OF THE COMMITTEE ON MOTION PICTURES AND SOUND RECORDING DEVICES IN INSTRUCTION OF PSYCHOLOGY

To the Council of Directors and Members of the American Psychological Association:

Your Committee reports 4 types of activity in which it has been engaged during the past year:

1. A manual on the production of research and instructional films in psychology, prepared under the auspices of the Committee by Mr. Harold Stone, Dr. Willard Valentine, and Dr. Walter Miles, has been completed. It has been accepted for publication in the *Psychological Bulletin*. Inasmuch as it contains useful information relating to problems encountered in motion picture photography, it may be regarded as a part of this report.

2. As a second activity of the Committee, reprints of Dr. Lester Beck's publication, "A review of sixteen-millimeter films in psychology and allied sciences," have been sent on request to Members of the Association. A few copies are still available for distribution. To bring the list of films to date, Dr. Beck has recently completed "A second review of sixteen-millimeter films in psychology and allied sciences." It contains more than 200 films, produced in the United States and abroad, with a list of film catalogues, sources, and periodicals.

3. In a preliminary approach to the problem of satisfactory standards for psychological films, the Committee has made a study of possible sources of improvement in motion pictures. The following suggestions are expressions of the individual members of the Committee:

a. In those films with poor photography, the difficulty generally lies in insufficient amounts of light or incorrect angles of lighting. Careful planning may eliminate this trouble and also the frequent problems of poor photographic emphasis, poor composition, and unsuitable length of scenes.

b. Added attention should be given to the clarity of sequences, which could be improved by more skilful handling of both titles and action. For general exhibition purposes, titles should avoid technical expressions. They should be frequent and complete, but the captions should not be disproportionate to the action portrayed. Titles should direct attention to the outstanding feature of the scene they describe.

c. Frequently, more repetition than necessary is found in psychological films. To eliminate the possibility that the behavior in question will be thought atypical, psychologists often feel that they must repeat 1 type of observation a number of times. Unless variability is being stressed, 1 scene, with a title indicating that the behavior is typical, should often be sufficient to prove the point. Exhibitors could avoid much unnecessary length also, if they would subject their films to the criticism of a small group before putting them into final shape for general showing. In the process of making a motion picture, the producer frequently comes to believe that each foot he has filmed is valuable. The opinions of individuals not so closely connected with making the film are valuable in indicating the relative importance of various parts.

d. In animal pictures, the impression of cruelty or unnecessary pain should be eliminated. Captions indicating the use of anaesthetics and precautions against discomfort are useful in this regard.

e. Care should be taken to see that the subject is suitable for film treatment, so that a moving picture will lead to added interest in the problem, rather than to disappointment in the topic and doubt as to its value.

f. It is wise to state, in the preliminary caption, whether a picture is of research or instructional nature. Since the purpose of research films is to give the actual results of experimentation, they must observe all the conditions necessary for accurate reporting of data. A demonstration film, on the other hand, aims not primarily to repeat an observation, but to teach its significance. Consequently, the instructional film may justifiably use any methods of dramatization which will help in the realization of its aim. Because of this difference, the 2 types of film cannot be judged by the same standards.

g. In line with the Association's policy of guarding against unsuitable material at its sessions by requiring an abstract of each paper to be considered for the Program, we should set standards for the films that are shown at the general meetings. Discretion must be used in showing motion pictures of definite political import or of shocking nature.

In an effort to help exhibitors locate the weak spots in their films, Mr. Harold Stone has adapted a rating scale which will be given preliminary use at this year's Annual Meeting. After revision, it is planned to make it available to all exhibitors for possible use before the next Annual Meeting.

4. The fourth type of activity has consisted of highly specialized work with motion picture photography, sponsored by the Committee or undertaken directly by its members. The coöperation of Dr. F. L. Dimmick has been secured for a study of the fidelity of color recording under various light conditions with different types of film. By means of a new supersensitive high-speed film, Dr. C. Landis and Dr. W. A. Hunt have been able to secure an effective speed of 3300 frames per second for minute analysis of behavior patterns. Also, Dr. Hunt has found, in the system using polaroid light, possibilities of using 3-dimensional films for

research and teaching. This method permits pictures in full color. Also, it should be stressed that the amateur production of 16-mm. sound films is now possible with a number of machines. Members of the Association are urged to work with sound recording. Electrical transcriptions of sound can be made simply and inexpensively with a large number of machines and have many possibilities for work in speech cases, music, and similar fields.

Respectfully submitted,

L. CARMICHAEL
E. A. DOLL
W. A. HUNT
W. R. MILES
W. VALENTINE
M. METFESSEL, *Chairman*

REPORT OF THE COMMITTEE ON PSYCHOLOGY AND THE PUBLIC SERVICE

To the Council of Directors and Members of the American Psychological Association:

The Committee on Psychology and the Public Service is glad to state that during the past year there has been more consideration given to psychology in the public service than there has been before in the last 20 years. There are more positions pending today in which a psychological background will be required than at any time previously.

At this writing a Federal Civil Service examination register is being set up for Junior Examination Assistant positions for which 20 hours of psychology and 1 course in tests and measurements are required. It is estimated that a number of appointments will be made from this register.

During the coming year the Committee will be greatly helped in its work if Members of the American Psychological Association would watch the state and municipal civil service announcements of examinations to be held for positions in the psychological and public administration fields. The announcements for such examinations should not fail to name psychology as a major educational requirement. They should specifically require credits in psychology. Any member who is interested in reviewing specific federal announcements should so inform the Committee.

Attention is invited to the fact that in New York State the positions of psychologist and of psychiatrist in practically all penal institutions in the state have been abolished in an economy drive. This action is directly against the proved concepts of the importance of psychological study and treatment in the rehabilitation of prisoners. It is doubly serious because of the fact that New York State has been regarded as a pioneer in penological work, and the action may result in other states taking the same steps. The question arises as to whether the Committee on Psychology and the Public Service, or the Psychological Corporation, should officially look into the New York situation and similar situations.

It is specifically requested that Members of the American Psychological Association who are interested notify members of the Committee of local, state, and federal announcements for positions which should be filled by a psychologist which now eliminate psychology, or of any situations which they feel the Committee should look into.

Respectfully submitted,
HAROLD E. BURTT
WALTER R. MILES
L. J. O'ROURKE, *Chairman*

REPORT OF THE COMMITTEE ON PRESS RELATIONS

To the Council of Directors and Members of the American Psychological Association:

At the present time the Association has no completely satisfactory arrangement for supplying the press with information regarding its activities or the activities of its Members. Papers presented at annual meetings are reported to the public largely through the independent efforts of the Associated Press, represented by Mr. Howard Blakeslee, and Science Service, represented by Miss Marjorie van de Water. In 1938 and 1939 the "Calls for Papers" have included requests for complete copies to be turned over to the press. In 1938, 39 papers were made available before delivery. In addition, local committees for the past 3 years have coöperated so far as possible with the local press. In the opinion of the Committee this very brief experience has proved the value of coöperation, but has clearly shown the inadequacy of present techniques.

Since the support of psychological research is coming to depend more and more upon a favorable public opinion and upon the opinion of administrators of research funds, who are to some extent influenced by nonprofessional reports, the value of an accurate and balanced public representation of psychological work is obvious. In view of the fact that the 2 news services already mentioned maintain writers especially trained to handle psychological material, the task of the Association is relatively simple. So far as the national press is concerned, all that need be done is to provide long abstracts or, preferably, complete copies of papers some time in advance of the annual meetings. The Committee recommends, therefore, that abstracts of at least 1000 words and, preferably, complete copies of papers, in triplicate, be required at the time of submission of papers to the Program Committee. The Program Committee could make use of this material in judging papers, where this is necessary, and 1 copy would then be turned over to the Associated Press and 1 to Science Service. The remaining copy would be held by the Secretary and made available to any accredited press service which could arrange to have a duplicate made. In order to encourage early preparation of complete drafts it is also recommended that the presentation of a paper be allowed even when it has appeared in a journal at any time during the 3 months preceding the meeting. This provision takes into account the increased speed of publication and the fact that the journals are not widely read during the summer months.

The Committee does not feel that coöperation with the local press is desirable beyond any arrangement that may be made by local committees. Local newspapers seldom have trained scientific writers and hence require a kind of copy that could be prepared only at considerable cost. The copies of papers to be retained by the Secretary could be placed at the disposal of the press at the local headquarters during the meetings.

The proposal of Science Service, which your Committee was instructed to investigate, has for the moment been abandoned because of lack of coöperation among the societies to which it was submitted. It provided for setting up a central publicity bureau for all the principal scientific societies which would prepare press releases throughout the year concerning the work of members of the supporting societies. A staff of experts, trained in scientific fields and in the techniques of newspaper writing, would be required and, whether carried out by a central bureau or by a single society, such an active form of publicity would involve a sizable expenditure. The Committee recommends that, for the present, the Association may advantageously utilize the expert service now provided by the 2 principal scientific news services through the coöperation suggested above. The opinion of the Members of the Association concerning a more active policy might profitably be ascertained.

Respectfully submitted,

Horace B. English
Donald G. Paterson
B. Frederic Skinner, *Chairman*

REPORT OF THE COMMITTEE ON DISPLACED FOREIGN PSYCHOLOGISTS

To the Council of Directors and Members of the American Psychological Association:

This Committee was appointed by the Association's President in September, 1938, to survey the problem of psychologists displaced from their positions and livelihood in other countries and seeking asylum and professional opportunities elsewhere. The Committee had no more than initiated its survey, however, when it was besieged with requests from individuals and from organizations to undertake active work in behalf of displaced foreign psychologists. In view of the clear need for an agency which could envisage the work as part of a widespread unemployment problem involving American psychologists as well, the emergency nature of many of the cases, and the deplorable waste of talent that might be contributing to the "advancement of psychology," the Committee broadened its scope.

The Committee has endeavored to find "non-competitive" positions for foreign psychologists and has been successful in a limited number of cases. There have also been other results whose value may be apparent later, and which may be serviceable in developing more outlets for domestic as well as foreign psychologists and in improving the quality of community services. It seems evident, indeed, that the "case work" which was conducted in response to an urgent need has contributed more than any other activity of the Committee toward the assigned survey of the problem as it now exists.

The Committee has engaged in the following activities during the year:

1. Coöperative relationships have been established with the various agencies for refugee aid, both in the United States and abroad. These have referred a high proportion of the cases known to the Committee; correspondingly, this Committee has referred cases to them for direct relief and other special services.

2. Curriculae vitae have been collected and summarized for about 220 displaced psychologists and scholars in related fields. This number includes 45 who have medical training, 28 philosophers, and 35 miscellaneous cases (including several students whose training is not complete, and a number of scholars with prominent psychological interests, but from other fields—law, biology, music, etc.). Curriculae vitae are available on request; many have been copied and transmitted to coöperating agencies or individuals.

3. In March, 1939, a confidential memorandum was issued, entitled, "List of Displaced Foreign Scholars." It included 111 cases for whom adequate reports were then available in the files of the Committee, and contained descriptive data regarding previous training and professional connections, family situation, present location and professional connections, if any, languages spoken, main fields of work, main publications, and professional references. Approximately 500 copies were circulated, with a request for coöperation, among members of the A.P.A. and other interested individuals and agencies.

4. Most of the displaced scholars who have reached this country have been interviewed by one or more members of the Committee—some of them on numerous occasions. By June 1, 1939, we were thus in touch with approximately 76 individuals—50 psychologists, 12 medically-trained men and women, 6 philosophers, and 8 miscellaneous cases, or about a third of the total group. Extensive correspondence has been carried on with a number of the cases who are still in the totalitarian countries or in other countries where they are awaiting an opportunity to emigrate.

5. In addition to assistance aimed directly toward professional resettlement, members of the Committee have rendered many incidental services which have doubtless contributed to the morale of scholars who are going through a trying period of readjustment. These services have included guidance in problems of adjustment to the culture, collaboration in research, finding translators for written productions, securing university library privileges, providing opportunities for lectures, arranging for attendance at (and transportation to) professional meetings, etc.

6. Correspondence and interviews have been initiated with institutions and individuals throughout the country with a view to placement of émigrés. This has resulted in some placements, and has also yielded vital information regarding the kinds of work in which there is an oversupply

of talent (in terms of current demand) and in which there are opportunities for expansion.

7. Placements in which the Committee has coöperated have been chiefly of 2 kinds: (1) temporary internships in schools, state institutions, clinics, or hospitals which would enable the individual to be self-sustaining while acquiring American methods and adjusting to a new culture; (2) temporary research and teaching positions which serve somewhat the same purpose, but have the further possibility of opening up new opportunities in psychology at the end of the subsidized probationary period. The Committee has successfully coöperated in 4 internships or volunteer clinical posts (with several additional cases pending); in 9 teaching and research positions—again with several additional cases pending; in 10 temporary placements which have provided professional stimulation as well as " relief " to individuals whose financial status was highly precarious; in 2 scholarships for further university training in special fields. In addition, positions of at least temporary duration have been found during the year through other agencies for 13 of the American residents known to the Committee.

Summing these placements gives a total of 38, of which 8 represent successive placements of the same individuals, making 30 individuals altogether. The point deserves emphasis that in the majority of cases the placements were not competing with the placement of American scholars. The internships provided subsistence, but little or no salary, in institutions able to provide living accommodations without seriously affecting their budget overhead. The " temporary positions " were of brief duration and served the main purpose of revealing the qualifications of those engaged in the work. Of the 8 individuals (representing 9 placements) in whose teaching or research positions the Committee has coöperated, only 4 were placed in positions paid out of regular institutional budgets. Even these cases were scarcely " competitive," for the qualifications of the scholars were so high and so unique that positions were virtually " created " to make room for them. In the other 4 cases funds were obtained for small research stipends; in 3 of these the funds were especially earmarked for émigré aid. In only 3 cases during the year has an extra-quota (teaching) invitation to this country been secured for a psychologist still in Europe. Although attempts were made to find academic invitations for several scholars whose cases were most urgent (*e.g.* men who were interned in concentration camps), there was strong reluctance on the part of university administrators to make appointments sight-unseen.

8. In prospecting for fields of professional work in need of development and promotion the Committee discovered several promising possibilities—among them the field of clinical work for the deaf, and social work for dependent children. This has led to negotiations (not yet completed) on behalf of several displaced foreign psychologists. In developing such possibilities in the future it would seem highly desirable to keep in view the availability of American, as well as foreign, psychologists.

9. Upon assuming a large amount of detailed work which it had not

originally planned to undertake, the Committee faced the necessity of raising funds. Up to June 30, 1939, receipts and disbursements were as follows:

Receipts

National Coördinating Committee	$100.00	
A.P.A.	50.00	
Regional Associations of Psychologists	100.00	
Private contributions	255.50	
Lecture proceeds	76.00	
Total Receipts		$581.50

Attention should be called to the last item under "Receipts," which represents the proceeds of a scientific lecture given under Committee auspices by an interested A.P.A. Member, who conceived this plan for raising funds.

Disbursements

Secretarial assistance	$156.00		
Professional assistance	98.00		
Travel expenses	49.82		
Postage, stationery, supplies	117.21		
Telephone, telegrams, cables	42.11		
Mimeographing	29.00		
Incidental	26.10		
Total Disbursements		$518.24	
Balance			$63.26

* * * * * *

The Committee's list of scholars is not complete, particularly for the countries in which racial and religious persecution has only recently made emigration vitally necessary. For Germany (including Austria) the list is probably fairly complete for scholars of professorial and dozent rank; far less complete for applied or clinical psychologists, assistants, etc. The German and Austrian scholars constitute about 80% of the total list.

Little more than a third of all 220 cases known to the Committee have secured even a temporary means of earning a livelihood; a few of the unemployed are living for the time being on their own funds, or through the help of relatives and friends, and many are living through week-by-week relief from refugee aid committees. Of the group of 76 who have reached this country, about half have found at least temporary employment (including "subsistence" internships), but this employment is usually very inadequate and poorly paid in terms of the ability and preparation of the incumbents. In only 5 or 6 cases have positions been obtained that are appropriate.

Only a very rough prediction is possible as to the rate at which refugee psychologists will continue to enter this country as quota-immigrants. (The field for "extra-quota" invitations being virtually at the saturation point, this mode of entering the United States will probably occur with increasing rarity in the future.) If immigration quotas remain the same, it is reasonable to expect the proportion of professionally trained persons to remain about the same, which may possibly mean 25 psychologists per year for some years to come.

Recommendations

Although there is clearly a present scarcity of positions in proportion to available personnel, in terms of research, educational, and community needs, there is no oversupply of psychologists. The unusual influx of professionally trained people following upon emergency conditions in Europe increases the disparity between available men and available jobs and contributes to a more general awareness of the problem. This may have a beneficial outcome in the long run if it serves as a stimulus toward a rational plan for supporting, distributing, and conserving talent.

As the problem of utilizing foreign scholars resident in this country is inseparable from that of utilizing American psychologists, it is recommended that a general committee be established under the auspices of the American Psychological Association, if possible with the coöperation of the American Association for Applied Psychology, for handling matters of personnel, promotion of professional opportunities for work, and public relations. To be effective such a committee should have widely scattered local sub-committees, and a part-time or full-time paid executive of the highest scientific reputation.

It is further recommended that a sub-committee on displaced foreign psychologists be appointed to work in close coördination with the general committee, the sub-committee to consist of those members of the present Committee who are interested in the specialized problems of the émigré, and of other members of the A.P.A. whose interest in these problems has been constructively expressed during the current year.

Respectfully submitted,

Luton Ackerson
Gordon W. Allport
W. E. Blatz
D. B. Klein
Gardner Murphy
A. T. Poffenberger
George D. Stoddard
Edward C. Tolman
Max Wertheimer
Barbara S. Burks, *Chairman*

Report of the Advisory Committee on the *Psychological Index*

To the Council of Directors and Members of the American Psychological Association:

The status of the Psychological Index Project, for which an Advisory Committee was appointed, is fully set forth in a report published in the *Psychological Bulletin*, 1939, **36**, 477–487. The Advisory Committee has rendered useful service, although it has been handicapped by lack of provision for one or two necessary physical meetings. Questions that have called for counsel from the Committee have been dealt with mainly by mail. On one occasion Dr. Willoughby spent a full day in conference with the Project staff, discussed procedure in detail, and was very helpful in the making of several important decisions.

Although the most critical period has passed, there will occur many occasions during the next few months when the advice of your Committee will undoubtedly be helpful. It is recommended, therefore, that the Committee be continued with its personnel unchanged for another year. Its continuance will be appreciated by the Project staff.

Respectfully submitted,

K. M. DALLENBACH
C. M. LOUTTIT
R. R. WILLOUGHBY
A. T. POFFENBERGER, *Chairman*

REPORT OF THE INVESTMENT COMMITTEE

To the Council of Directors and Members of the American Psychological Association:

On December 6, 1938, the Committee directed the sale of the U. S. Treasury obligations that the Association has held for a period of years, and the money was distributed among the various savings accounts of the Association of which there are now 8 in sound institutions in Providence, Princeton, Columbus, New York, and San Francisco. The average rate of interest earned by these funds is 2%. The income derived from the holdings of the Association is approximately $600 per year.

Respectfully submitted,

LEONARD CARMICHAEL
SAMUEL W. FERNBERGER
WILLARD L. VALENTINE, *Chairman*

REPORT OF THE REPRESENTATIVES OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION TO THE COUNCIL OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

To the Council of Directors and Members of the American Psychological Association:

Your representatives on the Council of the American Association for the Advancement of Science wish to report that they attended the meetings of this Council and participated in its deliberations. Two matters of business that have been considered during the past year seem worthy of review here: The first concerns a proposed Council on Human Relations; the second, the relationship of the American Association to the British Association.

Proposed Council on Human Relations

A special committee consisting of Drs. H. B. English, O. W. Caldwell, W. M. Krogman, E. P. Hutchinson, and Leonard Carmichael submitted the following proposal for a Council on Human Relations:

"In conformity with the Association's deep and long continued interest in conservation and the relation of science to education and human welfare, and at the suggestion of the U. S. Forest Service the Executive Committee named a special committee to consider the desirability of establishing in the American Association for the Advancement of Science an Advisory Council on Human Relations. That special committee, consisting of H. B. English, E. P. Hutchinson, W. M. Krogman, Otis W. Caldwell, and Leonard Carmichael, has submitted a plan which has been examined by the Executive Committee and is now presented to the Council with the recommendation that it be approved.

"The proposed plan is as follows:

"1. The A.A.A.S. shall appoint an Advisory Council on Human Relations (hereinafter known as the Council) the function of which shall be in general to act as a central advisory and coördinating body for research into the human-relations problems of reforestation and soil conservation, and such other problems as may arise.

"2. The Council shall consist of not more than 10 members, and shall be authorized for a period of not longer than 5 years.

"3. Two members shall be nominated by the governing councils of each of the following societies, and the selection and the appointment of 10 persons shall be by the Executive Committee and Council of the A.A.A.S.: the Academy Conference; the American Council on Education; the Social Science Research Council; the National Research Council; the American Association for Applied Psychology; the American Psychological Association; the American Sociological Society; the Rural Sociological Society; the American Anthropological Association; and the Society of American Foresters.

"4. In the event of retirement of any member of the Council, the vacancy shall be filled by action of the Executive Committee of the A.A.A.S.

"5. The present special committee shall continue until the Council has been organized.

"6. The duties of the Council shall be:

"a. To consider and make recommendations concerning the work of research projects proposed by government or other public agencies, and to make the conclusions available.

"b. To advise concerning proper scientific techniques to be employed in such coöperative social science investigations.

"c. To act in an advisory capacity in the choice of qualified personnel to carry out such research projects in human relations as are requested from time to time, and to advise on personnel training and public education pertaining to the human-relations aspect of forest or other problems.

"d. To give counsel and criticism concerning the preparation of results for publication.

"7. The Council shall be explicitly enjoined from engaging in activities other than those specified above, except with the approval of the Executive Committee of the A.A.A.S.

"8. The Human Relations Council shall report annually to the Executive Committee of the A.A.A.S."

The Executive Committee recommended that the Council accept the report and authorize the formulation of a Council on Human Relations.

Relationship With the British Association

Dr. Birkhoff reported on conferences of the representatives of the American Association with officers of the British Association. The conferences resulted in 2 proposals, which were adopted by the General Committee of the British Association at its meeting in Cambridge in August, 1938:

a. Each of the associations, on alternate years, shall invite a distinguished representative of the other association to deliver a principal address at the following meetings: the annual meeting in the case of the British Association and the summer meeting in the case of the American Association.

b. The American Association shall elect as honorary members, during their respective terms of office, the members of the General Committee of the British Association, and send to each of these honorary members the general programs of its meetings and copies of *Science* containing the announcements and reports of its meetings. The British Association, on its part, will elect as honorary members, for their respective terms of office, the members of the Executive Committee and the Secretaries of the Sections of the American Association, and send to each of these honorary members announcements and reports of its meetings. The proposals were adopted and the Permanent Secretary was instructed to take the necessary steps on the part of the American Association to put them into effect.

During the year the Permanent Secretary, Dr. F. R. Moulton, following the advice of the Executive Committee, has been engaged in an effort to develop a better plan for the annual election of the President of the American Association for the Advancement of Science. Your representatives have taken part in this discussion and have proposed a scheme that is essentially identical with the one now followed by the American Psychological Association.

Respectfully submitted,

WALTER R. MILES
JOHN A. MCGEOCH

REPORT OF THE REPRESENTATIVE ON THE COUNCIL ON HUMAN RELATIONS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

To the Council of Directors and Members of the American Psychological Association:

At its Richmond meeting, December, 1938, the A.A.A.S., at the request of the U. S. Forest Service, voted to establish an "Advisory Council on Human Relations" and requested a number of national organizations, among them the A.P.A. and the A.A.A.P., to make nominations for membership therein. At a meeting April 23, 1939, the Executive

Committee of the A.A.A.S. elected Dr. Horace B. English, nominee of the A.P.A., and Dr. Douglas Fryer, nominee of the A.A.A.P.

The Council held its first meeting July 13 and 14 at Washington, D. C. After receiving a report from the Organizing Committee (which had also functioned as a temporary Council for 6 months and had initiated 2 projects), the Council organized by electing Dr. H. B. English as Chairman and Dr. C. E. Lively (rural sociologist from the University of Missouri) as Secretary. The chiefs of several sections in the Forest Service presented their problems of human relations, and possibilities of scientific help in solving these problems were given preliminary consideration.

First attention is being given to the problem of preventing man-made fires in the forests. At Pratt Institute, Dr. A. I. Bryan investigated the development of a curriculum project-unit for use in schools centering around such fire prevention. Under her direction, the teacher-training students in fine arts have prepared a definite course of study for each grade level, using fine arts and dramatics to integrate contributions from geography, natural science, civics, and economics.

At Ohio State University a group of psychologists under the chairmanship of Dr. H. B. English has undertaken the preparation of information and attitude scales dealing with forest conservation. These scales, *inter alia,* will be used to test the efficiency of the art-centered curricula prepared at Pratt Institute as well as the material released by the Education Division of the Forest Service.

The Forest Service has coöperated with Drs. Bryan and English in the preparation of a motion picture film representing human behavior in the forest as a concrete basis for testing. It is believed that this is the first film prepared expressly to provide a setting for testing attitudes.

These projects are aimed primarily at the forest visitor. Residents in or near the forest, however, are a more troublesome problem. Ignorance, illiteracy, and peculiar folkways combine with economic motivations to present a baffling problem to the conservationist. The Council has under consideration a number of proposals for field studies of this problem—studies utilizing some of the techniques of the anthropologist, the social worker, the clinical psychologist, and the market research analyst.

The Council does not, however, conduct, nor even supervise, research projects. (Dr. English's activity in the early projects is entirely independent of his membership on the Council.) Its purpose is truly advisory only. Indeed, it seems likely that in a majority of cases its function will be merely to advise where the best advice is to be found.

At present the Council is concerned with the Human Relations problems presented to it by the Forest Service which requested its appointment; but the authorization by the A.A.A.S. implies that it may play a very important rôle in developing the human relations aspect of public services and bureaus. Members of the Association may find themselves asked whether they can coöperate in various ways.

Respectfully submitted,
HORACE B. ENGLISH

REPORT OF THE REPRESENTATIVE OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION ON THE AMERICAN DOCUMENTATION INSTITUTE

To the Council of Directors and Members of the American Psychological Association:

Last year a detailed report was submitted covering the purposes and work of the newly organized American Documentation Institute, which consists of representatives from some 50 scientific and scholarly societies and is interested in the development of facilities for microfilming documents, periodicals, books, etc. and making their contents more widely available to research workers. During the past year the services of the Institute have been utilized by many individuals and institutions. The annual meeting was held in Washington, January 26, 1939. In addition to reports on the activities of the Institute there was discussion of the extension of its services, especially to cover doctoral dissertations and other manuscript materials. Of interest to psychologists is a service known as the Auxiliary Publication Service, whereby original data, tables, etc. may be deposited with the Institute, copied on film, and made available to other workers, supplementary to briefer publication of the scientific paper itself in regular journals. The Bibliofilm Service of the Institute, through which many extracts from books and periodicals in the government libraries and the Library of Congress can be made available to scholars for a small fee, is now widely used by workers in many fields.

A circular describing the work of the Institute and information concerning its services may be obtained by addressing the Secretary, American Documentation Institute, care of Science Service, 2101 Constitution Avenue, Washington, D. C.

Respectfully submitted,

JOHN E. ANDERSON

REPORT OF THE DELEGATES OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION TO THE INTER-SOCIETY COLOR COUNCIL FOR 1939

To the Council of Directors and Members of the American Psychological Association:

(1) At its 1938 meeting the American Psychological Association voted to act as co-sponsor of the 1939 annual meeting of the Inter-Society Color Council on February 23 in New York City. At the sponsored session held in the afternoon, a symposium dealing with various aspects of the problem of color tolerances was presented. Dr. Sidney M. Newhall, a member of the A.P.A. delegation, was in charge of arranging the symposium. Three of the 7 papers were presented by the following Members of the Association: Dr. Edwin G. Boring, Dr. Sidney M. Newhall, Dr. Harry Helson. Dr. Forrest L. Dimmick presided. At the popular session in the evening, Dr. Michael J. Zigler represented the A.P.A. with a paper on "Color in Psychology." The program of the

meetings was announced in the official publication of the several member societies, and the papers of the technical session on color tolerances have been published in the July issue of the *American Journal of Psychology*. Copies of the symposium will be furnished to all members of the delegation. Other members of the Association can obtain copies by communicating with the Chairman of the delegation.

(2) The "dictionary" of color terms as they are used within the various member bodies will appear shortly in mimeographed form. Over 300 terms (not color names) are listed, many with 2 or more definitions. Copies will be sent to members of the delegation and can be obtained by others who are interested.

(3) A system of color names, after having gone through 22 revisions, has been adopted by the Council for application to pharmaceutical substances. This limited adoption is due to the fact that the problem was first undertaken at the instance of the U. S. Pharmacopoeia and has been accepted by them as suitable and adequate. The system was made to conform with usage in general, so far as was possible, and the Council is now examining its acceptability in other fields.

(4) The Council has adopted a method of defining Theatrical gelatines, which consists of abbreviated spectral distributions expressed in numbers of 7 digits. Every digit indicates the degree of transmission of the gelatine at a particular wave-length. The 7 wave-lengths are chosen to be sufficiently representative of the total curve for practical purposes. The method of designation is applicable to transmitting media for which such a degree of specification is adequate.

(5) The Council has prepared a large scale I.C.I. mixture diagram for distribution at a nominal price.

(6) Five issues of the "News-Letter" have appeared during the year. They have carried a wide range of color information and have been the means of enlisting increasing interest in the Council. Copies have been distributed to all delegates.

(7) Membership in the Council has been increased to 10 national associations and 50 individual members. The number of individual members is increasing continuously and steps are being taken to obtain the coöperation of additional national societies.

(8) Plans are under way for the 1940 meeting. The Technical Association of the Pulp and Paper Industry has undertaken to sponsor the technical session which will have for its topic the practical applications of spectrophotometry.

(9) The term of the present Executive Committee, which has been headed by the Chairman of your delegation, expires January 1. The A.P.A. will be represented on the new Executive Committee by Dr. Sidney M. Newhall.

Your delegation has taken an increasingly active part in the work of the Council. As a result, keen interest has been aroused in the contributions of psychology to the problems of color. On the other hand, activity in color on the part of psychologists themselves has been increased both among the delegation and among others who have been brought into contact with special aspects of the field through the medium of the Color Council.

Recommendations

Your delegation to the Inter-Society Color Council recommends:

(1) that the A.P.A. continue its membership in the Inter-Society Color Council;

(2) that the present vacancy on the delegation be filled by the election of Dr. Louise L. Sloan.

Respectfully submitted,

FORREST L. DIMMICK, *Chairman*
SIDNEY M. NEWHALL, *Voting Delegate*
MICHAEL J. ZIGLER, *Voting Delegate*
FRANK A. GELDARD
CLARENCE H. GRAHAM
JOY P. GUILFORD
HARRY HELSON
THEODORE F. KARWOSKI
ELSIE MURRAY

REPORT OF THE REPRESENTATIVE ON THE NATIONAL OCCUPATIONAL CONFERENCE, 1938–1939

To the Council of Directors and Members of the American Psychological Association:

The National Occupational Conference has decided to discontinue its activities and disband its organization on September 30, 1939. By that time, several of its major functions will have been transferred to more permanent agencies.

The clearinghouse and information service has been taken over by the Occupational Information and Guidance Service, United States Office of Education, Washington, D. C.

Occupations, the Vocational Guidance Magazine is now being published by the National Vocational Guidance Association, Inc., with headquarters at Teachers College, Columbia University, 425 West 123rd Street, New York City.

The *Occupational Index* and the occupational abstracts are now being published by Occupational Index, Inc., at New York University, Washington Square East, New York City.

Respectfully submitted,

PERCIVAL M. SYMONDS

REPORT OF THE REPRESENTATIVE TO THE NATIONAL RESEARCH COUNCIL

To the Council of Directors and Members of the American Psychological Association:

The annual meeting of the Division of Anthropology and Psychology of the National Research Council took place on April 22 in the building of the National Research Council in Washington. At this meeting reports of various committees of the Division were presented and routine action taken on the recommendations made.

In the writer's opinion, the most significant event of the year in the Council has been the work of the Committee on Problems of Neurotic Behavior. Under the able direction of Professor Walter S. Hunter, of Brown University, Chairman of the Division, the program of this Committee has progressed with Professor Walter R. Miles, of Yale University, as its effective Chairman. Working in coöperation with the Macy Foundation, the Committee has been able to promote the publication of a new journal, *Psychosomatic Medicine,* which has the purpose of bringing together studies which will contribute to the understanding of the organism as a whole in both somatic and psychological aspects. Many Members of the American Psychological Association have already received sample copies of this new journal. In successfully launching this journal, the remarkable achievement of Professor R. S. Woodworth in developing the Society for Research in Child Development under the sponsorship of the Division has been duplicated.

At the April meeting, a good deal of discussion was devoted to the possible contributions of the sciences of anthropology and psychology to the national government in time of emergency. The willingness of the professional members of the Division to aid in any such work was transmitted to the parent body of the National Research Council, the National Academy of Sciences, the agency designated by Congress to serve in this capacity.

As always in the past, the meetings of the Division have furthered coöperative thinking by psychologists and anthropologists on common problems.

Respectfully submitted,

LEONARD CARMICHAEL

REPORT OF REPRESENTATIVES ON THE SOCIAL SCIENCE RESEARCH COUNCIL

To the Council of Directors and Members of the American Psychological Association:

The 3 representatives from the A.P.A. on the S.S.R.C. for the year 1939–1940 are Mark A. May, A. T. Poffenberger, and R. M. Elliott. This report was written by Mr. May, the senior member of this group.

During the year the Council has devoted considerable time to the review and discussion of its major functions and objectives and to the activities of approach to them. At the annual fall meeting in September, 1938, the Chairman of the Problems and Policies Committee, Mr. House, reported at length on the study that this Committee had made of a previous analysis and review which had been made of the Council's functions and activities. Mr. House's report was the main topic of discussion both at the fall meeting in 1938 and at the spring meeting in 1939. Briefly, the purposes of the Council were stated as (1) the recruitment and training of research personnel, (2) the improvement and facilitation of research, and (3) the dissemination of results. The first of these objectives is approached mainly by the work of 2 Committees, 1 on Social Science Personnel, and 1 on Research Training. Carl Brigham, of Princeton University, is the Chairman of both of these Committees. The Committee on Social Science Personnel advocates the Council's program of pre-doctoral and post-doctoral fellowships. The Committee on Research Training was created at the fall meeting in 1938 for the purpose of studying procedures used in graduate and professional schools for the recruitment and training of social science personnel. Professor Brigham has submitted to the Council an extensive report based on a study of social science departments of the leading graduate schools of the United States. This report was used as the basis for discussion at a conference on training for research. The recommendation of the conference will not be considered by the Council until its fall meeting in 1939.

For the year 1938–1939 the Council made 26 fellowship awards, none of which went to a psychologist. For 1939–1940, 29 awards were made, and 1 was received by a psychologist and 2 by psychiatrists. The psychologist receiving the award is Dr. A. A. Campbell, Ph.D. Stanford, and instructor at Northwestern. He will spend the year in England. These awards are made partly on the basis of a competitive examination, partly on the nature of the training program that the applicant submits, and partly on the evidence of promise that the applicant shows of becoming a top-flight research worker. The small number of awards to psychologists is partly due to the fact that the number of applicants is relatively small and partly to their standing when compared with applicants from other disciplines.

A second major function of the Council is to facilitate better research in the social sciences. To this end it has engaged in 3 major types of activities: nuclear studies, seminar conferences, and the Committee on Appraisal. The first nuclear study was made by 3 psychologists on the topic of "Coöperation and Competition." The Committee consisted of G. W. Allport, Gardner Murphy, and Mark A. May. An extensive mimeographed report was gotten out by the Committee, and a monograph based on it was written by Mark A. May and Leonard Doob. Other nuclear studies of interest to psychologists are on crime and delinquency, acculturation, cultural hybrid, and methods of sampling in the social sciences. Copies of the published reports may be secured from the offices of the Council at 230 Park Avenue, New York City.

The facilitation of research by the conference-seminar method has received considerable attention. The method has been studied and discussed by the Council, and numerous conferences have been held. One that is of interest to psychologists was held recently at Princeton. It was attended by 7 psychologists who discussed the problem of the use of personality tests for the selection of government personnel for administrative positions.

The Council's Committee on Appraisal is endeavoring to improve research by selecting for critical analysis certain outstanding books or reports selected from the 7 disciplines that are represented on the Council. A book or a report has been selected for each of the disciplines except psychology. No selection was made here for the reason that the psychologists who were asked to nominate books or reports for review did not agree on any one. Professor A. T. Poffenberger, the psychologist on the Committee, carried on extensive correspondence with a selected group of psychologists who are interested in the social field for the purpose of finding some one study for review. This correspondence revealed a wide difference of opinion on the scientific merits of publications in social psychology.

Another activity of the Council for promoting research is its grants-in-aid program. For 1939–1940, 51 grants were made, of which 8 went to psychologists. The psychologists receiving grants for 1939–1940 are: L. M. Hawks, Illinois; E. E. Kuder, Letchworth Village; T. M. Newcomb, Bennington; W. O. Pierce, Columbia Grammar School; S. Rosenzweig, Worcester State Hospital; Ross Stagner, Akron; D. R. Disher, Florida State College for Women; and Evelyn Raskin, Randolph-Macon Woman's College.

The activities of the Council for approaching its third object, that of dissemination of research, have been for the most part exploratory. The matter has been under rather continuous discussion. A tentative plan has been made for specific activities.

The Council welcomes suggestions from members of its constituent societies on how research in the social fields may be more scientifically adequate and more socially useful. Members of the A.P.A. are invited to correspond either with their representatives on the Council or directly with R. T. Crane, the Executive Director of the Council.

Respectfully submitted,
MARK A. MAY

REPORT OF THE TREASURER AND BUSINESS MANAGER OF PUBLICATIONS

I am transmitting herewith the audited accounts of the American Psychological Association and its publications for the year January 1, 1938, to December 31, 1938.

During this year the balance sheet of the Association has shown a substantial increase over the preceding years. The picture of the various branches of the Association's activities is best revealed by the statement of income and expense by divisions:

The *Journal of Abnormal and Social Psychology* had a balance at the end of the year of $148.96; *Psychological Review,* $733.12; *Psychological Bulletin,* $358.51; *Psychological Monographs,* $1,183.67. There were deficits in the operation of the *Journal of Experimental Psychology* and of the *Psychological Abstracts.* In the case of the first-named journal, the deficit is $1,154.06. The deficit of $654.01 for the *Psychological Abstracts* is substantially below the estimate made by the Woodworth Committee in 1937, and the reserve fund is great enough to insure the continuance of this journal in its present form.

For the first time the accounts have been put on the accrual basis.

All earlier reports have been on a cash-disbursements basis without any attempt to tie up receipts and expenditures with the proper period in the operation of the financial affairs of the Association.

During this year there is a favorable relationship between the amounts budgeted and the amounts actually spent for the various operations of the Association.

The principal activity of the Business Office during 1938 was the preparation for centralizing the addressing operations in the Business Office, which has thereby taken over some of the functions that the printers had previously served. The full value of these activities will not be evident until the report for the year 1939 is made.

In an effort to increase the circulation of the *Psychological Abstracts* 1000 extra copies of the index issue were printed. In addition to the regular index material these copies contained descriptive material regarding the *Psychological Abstracts*. The copies were distributed gratis to 1000 libraries in this country and abroad. We estimate that the total charges were $133.00. So far as we have been able to ascertain, not a single new subscription can be traced to this project. It appears, then, that this type of advertising is not efficacious.

Respectfully submitted,

WILLARD L. VALENTINE, *Business Manager*

CONDENSED REPORT OF EXAMINATION

AMERICAN PSYCHOLOGICAL ASSOCIATION, INC., AND PSYCHOLOGICAL REVIEW COMPANY

Year Ended December 31, 1938

August 10, 1939

Auditor's Certificate

American Psychological Association, Inc.:

We have made an examination of the balance sheet of American Psychological Association, Inc., as at December 31, 1938, and of the statements of income and expense and net worth for the year ended at that date. In connection therewith, we examined or tested accounting records of the Association and other supporting evidence, and obtained information from officers and employees of the Association. We also made a general review of the accounting methods and of the operating and income accounts for the year and made certain test checks of the records of cash transactions and of data supporting the operating and income accounts, as hereinafter outlined.

The charter of Psychological Review Company, a wholly owned subsidiary, was cancelled August 3, 1938; however, on the records of the Association the assets and liabilities of that company were consolidated with those of the Association as at January 1, 1938, and the operations of the company for the period from January 1, 1938, to August 3, 1938, were included with the operations of the Association.

Prior to January 1, 1938, the records of the Association were kept on a "cash basis"; however, as at that date the Association changed its accounting policy and the records subsequently have been kept on an "accrual basis."

Condensed Comparative Balance Sheets

A summary of the balance sheets at January 1, 1938 (after consolidation of the assets and liabilities of Psychological Review Company), and December 31, 1938, follows:

Assets	Dec. 31, 1938	Jan. 1, 1938	Increase Decrease*
Cash	$68,115.72	$50,123.37	$17,992.35
U. S. Treasury Bonds	0	12,000.00	12,000.00*
Accounts receivable—net	133.67	386.44	252.77*
Inventories:			
Valuation placed on stock of back numbers of publications, etc.	5,500.00	5,500.00	0
	$73,749.39	$68,009.81	$5,739.58

Liabilities and Net Worth			
Accounts payable	$1,202.36	$1,525.07	$322.71*
Deferred income:			
Unexpired subscriptions	17,463.26	14,585.06	2,878.20
Reserved for special purposes	5,453.67	5,234.19	219.48
Net worth	49,630.10	46,665.49	2,964.61
	$73,749.39	$68,009.81	$5,739.58

The increase of $2,964.61 in the net worth of the Association is accounted for in the accompanying schedule of net worth.

Scope of Examination and Other Comments

The following comments are submitted in regard to the assets and liabilities, as set forth in the accompanying balance sheet, and to the extent of our examination:

Cash on demand deposit and in savings accounts, as shown in the balance sheet was confirmed to us by the depository banks and the amounts reported were reconciled with the balances shown on the Association's records. Cash in office funds was accounted for. We examined recorded cash transactions for the year to the extent of tracing the aggregate of receipts into bank deposits, as shown by bank statements on file, and examining cancelled bank checks as well as invoices, receipts, or other data on file in support of the disbursements through bank accounts.

Subscriptions and other accounts receivable were evidenced by listing the individual accounts, but we did not correspond with the recorded debtors.

Inventories included in the accompanying balance sheet represent the valuation placed on the Association's stock of back numbers of publications, etc. Similar inventories have been carried for several years in balance sheets of Psychological Review Company at the same valuation. We were advised by an officer of the Association that an attempt would be made during the year 1939 to determine an amount for such inventories which would more nearly reflect their value. We were further advised that physical inventories of back numbers were taken by the printers, with whom they were stored, during the month of October, 1938, and that subsequently perpetual inventory records were being maintained. A summary of such inventories is included elsewhere in this report. We noted that sales of single copies and back numbers for the years 1936, 1937, and 1938 amounted to $1,644.79, $2,594.78, and $1,849.33, respectively.

Provision was made in the accompanying balance sheet for all ascertained liabilities of the Association at December 31, 1938. The liability to authors of psychological monographs was evidenced by trial balance of the individual accounts, but we did not correspond for further confirmation.

Deferred income consists of the unexpired portion of subscriptions to various publications of the Association at December 31, 1938. We made a test check of the Association's computations with respect to these subscriptions.

Information submitted to us indicated that certain funds were reserved for specific purposes and were not to be considered a part of the general funds of the Association. Cash carried in a special account in Peoples Savings Bank in Providence amounted to $2,873.52 and represented the unexpended balance of subscriptions by members for the entertainment of foreign delegates to an international meeting in 1932 plus accumulated interest thereon to December 31, 1938. Under the terms of the gift whereby the Association acquired the *Journal of Abnormal and Social Psychology,* any surplus funds arising from its publication should be used solely for purposes of that journal. The amount of such surplus funds at December 31, 1938, was determined as follows:

Balance at January 1, 1938....................	$2,431.19
Net income from operations for year..........	148.96
Balance at December 31, 1938............	$2,580.15

ERNST & ERNST
Certified Public Accountants

BALANCE SHEET
AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.
December 31, 1938

ASSETS

Cash				
Demand deposit		$31,733.54		
Savings accounts ($2,873.52 reserved for special purposes)		36,375.22		
			$68,108.76	
Office cash funds			6.96	
				$68,115.72
Accounts Receivable				
For subscription to Journal of Abnormal and Social Psychology	$5.00			
For sales, reprints, etc.	140.54			
		$145.54		
Less reserve		72.77		
			$72.77	
From authors for printing costs, etc.			60.90	
				133.67
Inventories				
Valuation placed on stock of back numbers of publications, etc.				5,500.00
				$73,749.39

LIABILITIES AND NET WORTH

Accounts Payable		
To authors of Psychological Monographs		$1,202.36
Deferred Income		
Unexpired subscriptions to:		
Psychological Abstracts	$8,255.62	
Journal of Experimental Psychology	2,866.95	
Psychological Bulletin	2,652.97	
Psychological Review	2,144.54	
Journal of Abnormal and Social Psychology	1,543.18	
		17,463.26
Reserved for Special Purposes		
Ninth International Congress account	$2,873.52	
Surplus funds of Journal of Abnormal and Social Psychology	2,580.15	
		5,453.67
Net Worth		
Balance at December 31, 1938		49,630.10
		$73,749.39

NET WORTH

AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

Year Ended December 31, 1938

Balance at January 1, 1938		$41,165.49	
Excess of net assets of Psychological Review Company over the carrying amount of that Company's capital stock, on the records of the Association—representing valuation placed on stock of back numbers of publications, etc.		5,500.00	
			$46,665.49
Net income for the year		$3,184.09	
Less portion of net income reserved for special purposes:			
Interest on funds collected for entertainment of foreign delegates in 1932	$70.52		
Net income of Journal of Abnormal and Social Psychology	148.96		
		219.48	
			2,964.61
Balance at December 31, 1938			$49,630.10

INCOME AND EXPENSE

AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

Year Ended December 31, 1938

INCOME	Total	Treasurer's Office	Publications
Dues	$6,247.00	$6,247.00	
Subscriptions paid by Treasurer's Office	11,242.50		$11,242.50
Other subscriptions	17,375.29		17,375.29
Reprints and commissions	4,035.60		4,035.60
Sale of single copies	1,849.33		1,849.33
Profit on sale of U. S. Government Bonds	920.25	920.25	
Interest	1,139.72	1,073.95	65.77
Advertisements	575.80		575.80
Miscellaneous sales, refunds, etc	300.93	97.92	203.01
TOTAL INCOME	$43,686.42	$8,339.12	$35,347.30
EXPENSE			
Stipends to editors	$3,475.00		$3,475.00
Stipends to officers	1,800.00	$1,800.00	
Compensation to employees	4,508.53		4,508.53
Abstractors and translators	1,570.34		1,570.34
Payments to authors	522.88		522.88
Printing costs	21,394.38	529.15	20,865.23
Reprints	3,313.37	651.42	2,661.95
Yearbook, Annual Meeting, and committee expense	1,697.57	1,697.57	
Office and miscellaneous expense	1,983.25	1,093.08	890.17
Equipment purchased	237.01		237.01
TOTAL EXPENSE	$40,502.33	$5,771.22	$34,731.11
NET INCOME	$3,184.09	$2,567.90	$616.19
NET INCOME-EXPENSE BY DIVISIONS			
Treasurer's Office	$2,567.90		
Journal of Abnormal and Social Psychology	148.96		
Psychological Abstracts	654.01		
Psychological Review	733.12		
Psychological Bulletin	358.51		
Journal of Experimental Psychology	1,154.06		
Psychological Monographs	1,183.67		
	$3,184.09		

BUDGET FOR 1940

TREASURER'S OFFICE

AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

Estimated Income

Interest on bonds	
Interest on savings	$750.00
Profit on sale of bonds	
Dues	6,469.00
Sale of Yearbooks and Programs	100.00
Total	$7,319.00

Estimated Expenditures

Printing and supplies	$500.00
Postage and express	500.00
Telephone and telegraph	10.00
Reprints and Proceedings	250.00
Yearbook (plus postage)	1,000.00
Treasurer's Bond, Safe Deposit Box, P. O. Box	100.00
Secretary's stipend	1,500.00
Treasurer's stipend	400.00
Incidentals, Annual Meeting	200.00
Committee on Animal Experimentation	25.00
Apparatus exhibit	50.00
Inter-Society Color Council	25.00
Binding Yearbooks and Proceedings	10.00
Auditing accounts	400.00
Committee on Displaced Foreign Psychologists	200.00
Committee on Personnel, Promotion, and Public Relations	50.00
Miscellaneous	10.00
Total	$5,230.00

BOOK REVIEWS

BORING, E. G., LANGFELD, H. S., WELD, H. P., & collaborators. Introduction to psychology. New York: Wiley, 1939. Pp. xxii+652.

The present volume is essentially a revision of *Psychology: a factual textbook,* published under the same editorial supervision in 1935 and reviewed by the present writer in this journal (1935, **32**, 595–599). The current volume is also a factual text. It sets forth in clear, simple language the essentials which should not only be the subject matter of an elementary course in one of the sciences, but which should be at "the finger tips" of all advanced students of psychology. The Editors have not yet succumbed to an apparently prevalent idea that college students should be lured into psychology with a jazzed-up and discursive pap which invites no respect from the student, the psychologist, or the follower of another science.

The following are the authors and the respective chapters which they have contributed: Boring, Chapter 1, "The Nature of Man"; Cantril and Frederiksen, Chapter 2, "Social Functions of the Individual"; Katz, Chapter 3, "Personality"; Catherine Miles, Chapter 4, "Individual Differences in Personality" (including statistics); MacKinnon, Chapter 5, "Motivation"; Landis, Chapter 6, "Emotional and Affective Responses"; Carmichael, Chapter 7, "Response Mechanisms" (the nervous system); Langfeld, Chapter 8, "Response"; McGeoch, Chapters 9 and 10, "Learning and Retention"; Bray, Chapter 11, "Recollection and Imagination"; Humphrey, Chapter 12, "Thought"; Feldman and Weld, Chapter 13, "Perception"; MacLeod and Weld, Chapter 14, "Spatial Perception"; Tinker, Chapter 15, "Perception of Time and Movement"; Boring, Chapter 16, "Sensation" (including psychophysics and attributes); Purdy, Chapter 17, "Vision"; Wever, Chapter 18, "Audition"; and Dallenbach and Zigler, Chapter 19, "Smell, Taste and Somesthesis." Of these authors, the following are new: Cantril, Frederiksen, Miles, and MacLeod.

In order to facilitate a comparison of the two editions, I have indicated roughly in Table I the absolute and relative amounts of space devoted to each of the major topics. Although there has been a large amount of reshuffling, condensation, and expansion of topics, the major change in content has been the addition of chapters on social functions, motivation, and individual differences. The earlier chapter on the nature of psychology has been replaced with one on the nature of man. The topic of learning has been given two chapters instead of one, and the term recollection has been used to broaden the implication of imagination. The chapter on affection has been combined with that on emotion. The discussion of sensory intensity, which occupied 19 pages in the earlier book, is now included in the chapter on the general aspects of sensation. The discussions of time and movement are now combined; and there has been a drastic reduction in the amount of space allotted to taste, smell, and somesthesia. It will be noted in Table I that the absolute number of pages in the two editions are essentially the same for most of the topics included in both books.

Although there is little general agreement among teachers with respect to the order in which the topics of elementary psychology should be presented, custom has established two broad procedures: one develops the subject from the allegedly simple to the complex after the fashion of Wundt, Titchener, and the first Boring-Langfeld-Weld *Psychology*; the other develops the subject along what might be termed pedagogical lines, seeking to interest the student from the beginning by dealing with material more closely related to his daily experience than are the topics of the nervous system and the receptive processes. This latter method has been followed by James and by others after him, including the second Boring-Langfeld-Weld "Psychology." However, a vital pedagogical organization should not only begin with important and significant problems as viewed by the learner, but it should end with the discussion of other such problems. The present book is undeservedly weakened by having as its final chapter a discussion, excellent in its own right, of taste, smell, and somesthesia.

TABLE I

A Comparison of the BLW *Psychology* With the BLW *Introduction to Psychology*

Psychology			*Introduction*	
Pages	% of Space	Chapter of *Introduction*	Pages	% of Space
0	0	Social Functions	40	6
40	8	Personality (2 chapters)	98	15
0	0	Motivation	37	6
47	9	Emotions and Affections	40	6
27	5	Response Mechanisms	27	4
42	8	Response	40	6
44	8	Learning and Retention (2 chapters)	61	9
30	5	Recollection and Imagination	30	4
33	6	Thought	30	4
26	5	Perception	28	4
39	7	Spatial Perception	35	5
28	5	Time and Movement	21	3
40	7	Sensation (general)	24	3
45	8	Vision	42	6
38	7	Audition	39	6
48	9	Smell, Taste, Somesthesis	27	4

It seems to me that there is very little negative criticism to be offered of the contents of the book viewed as an elementary text, where incompleteness of treatment is inevitable. A few points, however, where future revision might be helpful, may be noted. (1) In the chapter dealing with the nervous system, the statement that there are "more than thirty pairs of spinal peripheral nerves" (p. 236) might well be changed to state simply that there are thirty-one pairs of such nerves. It would also be reasonable to expect a list of the chief cranial nerves with their functions and cortical connections, particularly since the volume devotes one-third of its space to sensory and perceptual functions. There should also be, I think, some brief suggestions concerning neural pathologies in relation to personality changes, since there is probably much more relevant material on this topic than on that of the neural basis of learning to which some space is devoted. (2) The discussion of learning itself does not reach back and tie onto the chapter on the nervous system. It might have done this and also have included some reference to the physiological mechanisms in conditioning. (3) The chapter on personality contains no reference to Freud and no account of the pathological and near-pathological conditions of personality other than 2 pages of comments on multiple personality. The only accounts of psychoanalysis are brief references in later chapters on emotion and recollection (dreams). (4) There is no relationship, except in the titles, between the chapter on personality and the following one on individual differences in personality. The latter chapter considers none of the aspects of behavior which the former chapter has said are the fundamental characteristics of personality. (5) Although many readers will miss any reference to Dewey's account of thinking in the chapter on thought, the most serious defect which this chapter has, in common with similar accounts in other psychological texts, is its complete failure to utilize the contributions made by logicians to the understanding and evaluation of thinking. The reasons for such neglect are complex, but they include, historically, such things as antagonism to philosophy, the belief that psychology deals only with experiential contents, and the supposition that logicians have dealt only with completed, and hence "dead," acts of thought. Surely, however, an account of the scientific, inductive methods of problem solution is an important part of a discussion of thinking. Both deduction and induction are aspects of behavior, analysis of which, even in an elementary psychology, can throw much light on such topics as straight and crooked thinking (as important as maze errors and successes), propaganda, and the adjustment of the individual to his environment. The conventional limitation of accounts of thinking to strictly "psychological" contributions contrasts greatly with the welcome extended to biological, physical, and sociological contributions when other problems of psychology are under discussion.

There are a great many complimentary statements that can be made about the present book. Some of these occur earlier in this review. The volume is well written, well edited, and the presentation of topics is authoritative. It is not above the level of comprehension of any college student who has the ability to pass the average beginning course in biology, physics, or chemistry. All in all, I believe the *Introduction to psychology* to be the outstanding text available to elementary students of the science.

WALTER S. HUNTER.

Brown University.

FREEMAN, E. Principles of general psychology. New York: Holt, 1939. Pp. xiv+530.

This book conforms quite rigidly to its title. While written from the background of experimental psychology, the emphasis throughout is upon general principles. It may appear to lack the concreteness of some modern textbooks which include a considerable amount of experimental data, charts, and tabular material; however, from the point of view of the reviewer, the emphasis upon principles probably affords the beginning student the most fundamental and substantial kind of introduction to psychology. It is not true that the average student is incapable of understanding principles if they are clearly presented. In this respect, the author has been admirably successful.

Psychology is defined as "the science of the experiences and behavior of organisms," and the *organismic* viewpoint of the relation between experiences and behavior is adopted. After a brief first chapter, entitled "Orientation," in which the subject matter and the data of psychology are discussed, the book is divided into two parts. Part I, "Principles of Behavior," deals with the following topics: heredity and environment; intelligence; organically determined differences; racial and cultural differences; socioeconomic status; language and number; science, technology, and invention; social conditioning; feelings; emotions; motivation: organically determined; motivation: socially determined; personality; the basis of learning; and the process of learning. "The Principles of Perception" is the caption adopted for Part II, which considers: general characteristics of perception; simpler visual perceptions; complex visual perceptions; organization; the other modalities of perception; imagery; and thinking.

The author states in his Preface that the book is not rigorously committed to any one school of psychology, and this is true. The author feels that "neither the status of psychology nor the requirements of teaching favor such commitment." The author has used Gestalt conceptions quite freely in dealing with the problems of learning and perception, where, according to him, they are peculiarly effective. According to the reviewer's bias, the chapters on learning would have profited by a more eclectic treatment and by the inclusion of more data on the conditions of learning, which are of both practical and theoretical interest. While Gestalt leanings are observable in a number of his interpretations, the author does not make a fetish of *patterns, configurations,* or *organization,* and has tried to avoid "fashionable jargon, sometimes used with alarming disregard for meaning."

The first part of the book includes considerable data from individual psychology; however, these data are presented with the view of affording a better understanding of the psychophysical organism rather than for their technological significance. The discussion of the relative merits of heredity and environment and the effects of race, culture, and socioeconomic conditions upon the intellectual status of the individual is critical and exceptionally well done. The source materials are well chosen, and principles are elucidated clearly and succinctly. Any student will profit greatly from a mastery of this material, for upon these topics there is a considerable amount of prejudice and careless thinking.

Ellis accepts the point of view that psychology is not a study of the nervous system, although neurological and physiological, as well as physical, concepts are employed when they are helpful in understanding psychological phenomena. This is a defensible position, and one may suspect that there is a tendency, among modern textbooks, away from the formal and rather divorced chapter on the nervous system. The author's contention that this neurological material has not facilitated our psychological insights as much as we supposed is undoubtedly true.

The reviewer is impressed by the fact that the author has a clear conception of the nature of science, and it is commendable that he shows how psychological facts are not only valuable *per se,* but how they may enrich and refine the conceptions of other sciences. Moreover, it is invaluable for any student to learn not only the factual materials of any given science, but also something of the nature of scientific inquiry. This text, carefully studied, should materially assist in the attainment of both of the aforementioned ends.

The following negative criticisms may be offered. In his discussion of pleasantness and unpleasantness, Ellis states categorically that there is no neutral state of indifference and that what may be regarded as neutrality is a state of mild pleasantness. Many writers do not hold to this point of view, and some observations indicate that there may be experiences which lack hedonic or affective tone.

In his discussion of thinking, it seems to the reviewer that the author places too much emphasis on imagery and not enough on the importance of language functions. It is true that the vehicle of thought may be an image, but thought is also carried on in terms of sensory and motor processes. Many competent observers are skeptical about imagery playing any important rôle in their thinking. And that "the thought processes of the most abstract philosopher are based upon imagery fully as much as are the thought processes of the illiterate" is an extremely doubtful assumption.

In his analysis of perception, Ellis states that "every perception is permeated with imagery." Certainly, not all psychologists would accept this statement at its face value. It would seem to say that imagery is necessary for a perception to have meaning. Imagery may or may not be present in perception. When present, it renders the meaning more precise, but the meaning may be carried in other than imagery terms.

Thinking is defined as "the process of problem solving." Would it not be better to state that it is a process of ideational problem solving? Problems may be solved by overt motor methods. But what we usually mean by thinking involves the substitution of ideational or symbolic processes for overt manipulation. Of course, the dichotomy is not absolute, but it is important if we are to think clearly about symbolic operations.

These, perhaps, are minor criticisms, and they certainly do not invalidate the book as a whole. The author is to be congratulated upon writing an excellent text dealing with the principles of psychology. The treatment of the subject is scholarly and dignified. It is the type of book that should be placed in the hands of the student. It is disheartening to the serious-minded psychologist that the demand for the "jazzy" and popular book is as great as it is. Give us more texts like the one under review and psychology will deserve a better reputation.

Paul L. Whitely.

Franklin and Marshall College.

Judd, C. H. Educational psychology. New York: Houghton Mifflin, 1939. Pp. xx+566.

The objective of the book has been to make clear to the beginning student of educational psychology the essentials of a large and somewhat abstract subject matter. Extreme simplification of difficult concepts rather than selection of material for its applied value appears to have been the rule by which the author sought to achieve this objective.

Part 1 deals with evolution and man in his relation to the animal kingdom; in Part 2 the social heritage of the child is considered; Part 3 discusses the development of personality; while Part 4 includes techniques of investigation and psychological findings of interest to educators.

Plates show evolution of the nervous system and areas of the human brain. The cerebrum is viewed as having associative functions: "One may say that the cerebrum is an organ of associative interpretation, not an organ of direct response" (p. 42). The alleged importance of great size of the human brain is stressed (pp. 42–48). Educational psychology, if it is to have applied value (and present trends suggest that it must), might well begin to question the practical use which teachers make of structural facts concerning phylogenetic development of the nervous system. The inclusion of such material in texts on educational psychology is based on traditional practice.

The temporal orientation of the author is suggested by the fact that one-fourth (23%) of the references, presented for supplementary reading and appended to the chapters, were published before 1920. This is further emphasized by the frequency of appearance of names of early psychologists: James's name is exhibited on 12 different pages, Wundt has 8 pages, Horace Mann has 7, and Spencer has 4. Few recent workers are cited (although Buswell, who leads this list, appears on 12 different pages). Such phrases as "because the mind recognizes" (p. 266) also seem to place the point of view in an earlier, more traditional frame of reference than the reader would expect when date of the publication is considered. The selection of material, system, and terminology strongly makes evident the author's early functionalistic viewpoint.

Some workable interpretation of the relationship between mind, body, and behavior, not to be found in the text, is necessary if the student is to profit from the discussion of behavior and adaptation, since "the kinds of behavior which an individual performs and the relation of behavior to the various levels of physiological organization and to consciousness are of prime importance for an understanding of human life and human development" (p. 59). It would seem that any attempt to describe adequately, for purposes other than philosophical discussion, the basic features of this tripartite relationship would fail.

In characterizing as inadequate the Lucretian theory of perception, which holds that impressions are stamped on the mind by the physical world, the authors says: "The physical world is, indeed, the source of sense impressions, but the world of mind is another world, in which associations, memories, and elaborate relational combinations are developed" (p. 126). At several other points the mind is also endowed with what would seem to be inexplicable powers—"the power of concentration of attention" (p. 253) and ". . . inner mental systems of classification of attributes or qualities" (p. 253). Aside from mental activity, on other pages the mind contains a kind of impalpable mental content. Conscious attitudes of the nature of the *Bewusstseinslagen* of the Würzburg school play their part when the author delves into epistemology. Meanings are established attitudes. "The subjective attitude is not only a very intimate, personal phase of each experience; it is the product and epitome of the individual's past contacts. . . The attitude persists as a permanent phase of consciousness but not as a complete memory" (p. 169).

Emphasis on mental activity and mental content, palpable and impalpable, may confuse, rather than enlighten, the student of applied educational psychology. Consideration of the development of organized behavior patterns, which might have been treated operationally, would have been more appropriate for a field which does not avowedly deal with epistemology, but which ". . . formulates as a result of its study certain principles which should be considered in organizing and administering schools" (p. 3). The author himself assumes that there is great need to stress to the student the importance of behavior and the futility of studying mental content when he states: "The change in emphasis from impressions to behavior was the result of the teachings of two great psychologists." They were Wundt and James, who ". . . called attention to the active side of human life as of major importance" (p. 60).

Studies in anthropology are drawn upon heavily in Part 2 (social heredity, 310 pages), in which is discussed the historical development and use of written numeral and alphabetical symbols, and language. Chapters dealing with spelling and primary reading and reading above the primary grades are more than survey discussions; they give methods of investigation, and the conclusions reached are amply fortified with experimental evidence, as are those of the chapters which take up the acquisition of language and of 'number ideas.' Much of the rest of Part 2 is discoursive. The chapter on vocational education and general education makes it clear that the distinction between vocational arts and liberal arts is an artifact; that continuity between curriculums and courses is lacking; that content of vocational courses and methods of instruction are chaotic. Vocational orientation and student personnel work are, unfortunately, not considered in the text, although reference is made to several excellent books on this subject.

Part 3 (78 pages) deals with personality. "A personality is the complete and unified outcome of all the developmental processes through which the individual has passed" (p. 447). Here the Binet scale, Spearman's two-factor theory, and factor analysis are each given a paragraph or two, while James's 'experimental studies of physiological retentiveness' are allotted 2 pages. To dissociation [". . . . the term calls attention to the fact that a process has set in which is the opposite of the organizing or associative process by which individuals attain normal mental powers" (p. 476)] is apportioned 20 pages.

The reviewer felt sure that Part 4 (40 pages) would yield information which would have applicational value to the prospective teacher. But there are only two chapters here which deal with methods of psychological investigation; and discipline, individual differences, supervised study, and lesson planning. Many different subjects, which range in nature from reaction-time measurements to Spencer on punishment, are mentioned. This last brief part is a survey of additional problems.

In writing this book the author's pen has often been guided by *lumen naturale* rather than by empirical evidence. While many of the topics have been well selected and treated in a manner commanding great respect (*e.g.* language, reading, acquisition of number facility), some have been included which have no value other than as general information. It is an impressive volume which will have cultural value for the prospective teacher; it contains relatively little which might be applied to practical problems of teaching or school administration.

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TRYON, R. C. Cluster analysis. Ann Arbor, Mich.: Edwards, 1939. Pp. viii+122.

Professor Tryon's aversion to the "mathematical magic" and the "ingenious but in many respects misleading activities of the factor analysts," together with his desire to obtain the fruits of factor analysis in some simple fashion, is responsible for his development of correlation profile analysis and orthometric analysis.

Profile analysis is the simpler technique. If the correlations between one variable and each of the others are plotted, a profile will be obtained. If such a profile is drawn for each variable, it will be possible by inspection to group the similar profiles together. This technique isolates those variables which have high intercolumnar correlations (which Tryon renames "congruence"). It is assumed that a factor (which Tryon renames an "operational unity") is responsible for each set of similar profiles. In an illustrative analysis of twenty variables, Tryon isolated five clusters of from three to five variables each. Correlations between the mean profiles of the five clusters ranged from —.57 to +.78.

Orthometric analysis (Tryon's new name for factor analysis) seeks clusters of tests and assumes that an operational unity is responsible for each cluster. Tryon has imposed the limitation that any group of tests shall not be analyzed into more than three (orthogonal) reference axes. This limitation sometimes makes it impossible to describe all the tests in a battery accurately, so subsequent analyses of subgroups of the tests may be necessary.

Detailed instructions are given for performing each of the steps involved in making the analyses. For each step there are statements of the purpose, the procedure, and the order of operations involved. The techniques present little computational difficulty.

Cluster analysis methods (including both profile analysis and orthometric analysis) seek groups of highly intercorrelating variables. Each operational unity will thus be a mixture or average of whatever is common to the tests in a given cluster. These operational unities will not, in general, be as stable and consistent as the factors extracted by the better factor methods.

Tryon is neither always consistent nor always well acquainted with the methods which he criticizes. He condemns "factor" analysis because the mathematics is likely to be misunderstood, or worse, adopted uncritically by many psychologists, and recommends "orthometric" analysis because a knowledge of the derivations is quite unnecessary.

In estimating the communality of a variable, Tryon averages the estimates made by two methods. He does not use Thurstone's method, Tryon explains, "because it is the hard way." Actually, both methods which Tryon uses are included in a list of eight suggested by Thurstone, and the *simpler* of his two methods is exactly the method commonly used and described by Thurstone and his students in a number of papers.

The problems attacked by orthometric analysis are the same as those attacked by factor analysis. The method of naming an operational unity is the same as that of naming a factor; the resultant names are sometimes identical. When one strips orthometric analysis of its new vocabulary, what remains is a not very rigorous method of isolating factors—a method basically similar to some of Thurstone's earlier, and discarded, attempts to solve the factor problem.

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The University of Chicago.

DANTZIG, T. Number: the language of science. (3rd ed., revised and enlarged.) New York: Macmillan, 1939. Pp. x+320.

The number 3, symbolizing the order of editions of this excellent treatment of such fascinating mathematical problems as the nature and history of number, number symbolism, continuity, and infinity, equals 26. For the third edition, aside from a few minor text changes, merely constitutes an addition to twenty-six brief appendices, increasing the number of pages from 260 to 320. Among these articles are: "On the Number Sense of Beast and Man," "How Archimedes Recorded Large Numbers," "On the Nature of Time," "The Occult in Geometry," "Squaring a Parabolic Segment," etc.

To psychologists, this work, which has been widely acclaimed both by the lay public and mathematicians, makes a double appeal. In the first place, here is a superior illustration of how a conventionally abstract and technical discipline can be made into a fascinating educational and cultural adventure. The author admirably succeeds in his intention of restoring to mathematics the cultural content of which the school curricula have robbed it.

Again, although the author intended the volume to be a popular exposition, his discussion is so penetrating as to challenge reflection upon fundamental mathematical issues. Of especial interest to the psychological student are the doctrines concerning symbols and symbol operations in language and thinking.

As the subtitle of his book indicates, Dantzig looks upon number as a sort of language. Numerical language is only a more precise system of symbols than everyday rhetorical algebra. "Nouns and phrases are but symbols of classes of objects, verbs symbolize relations, and sentences are but propositions connecting these classes" (p. 98). Because this venerable notion of language not only is a travesty upon language, but has also played havoc with a great deal of mathematical thinking (witness the controversies of the formalists and intuitionists), it deserves to be carefully, if briefly, considered.

Basically, the symbolic notion of language is static or reistic. It implies that language and mathematics consist primarily of handling things which represent other things. Speech is thus reduced to uttering word symbols substituting for things, while mathematics becomes a series of operations upon counters according to certain rules. The rules for ordering or relating symbols derive from either (a) mysterious intuitions, (b) equally mysterious "reasoning," or (c) arbitrary decision.

The necessary corrective here is to carry operations back to genuine interbehavior. When we do so, we can distinguish between speech, symbolization, and mathematical processes, on the one hand, and symbol-things as factors in such interaction (whether simple tools or records of results), on the other. This correction carries far enough to avoid palpable confusion of phenomena, though we employ a single name (language) to refer to them.

Language as speech (conversation) is interbehavior with things and persons by way of referring other persons to them, while symbolizing language is setting a sign to substitute for some object or process (records). Mathematical activities consist of interbehaving with relations, whether of the ordering or mensurating sort. Now it is clear that we have here three distinct forms of interbehavior which cannot be characterized as merely three species of symbol manipulation. In language as speech no symbols are essential. The necessary mutuality or conventionality of performance is not to be equated with symbols. Symbolizing interbehavior obviously involves symbols and, of course, such activities are essential in mathematical work because of the complexity of the activities involved. But here, too, we must distinguish between the activities of symbolizing, on the one hand, and ordering, calculating, inferring, implying, and other interbehavior with symbolized relations, on the other.

When we observe the indicated distinctions, it becomes plain that numbers are not merely symbols of mathematics and, because of the metric processes in science, the "language" of science. Numbers as symbols are only tools for measuring, computing, and recording. This, however, is only one of their identifications. Numbers are also records of acts of numbering and ascertaining, measuring, and relating relations. And can we forget that the term 'number,' as in the phrase "science of number," also refers to the complex activities of enumerating, calculating, measuring, and investigating relations themselves? Neither science nor mathematics, any more than language, is the manipulation of symbols. And certainly, neither mathematics nor science constitutes a language, nor is mathematics to be confused with science. Language, science, and mathematics consist of intricate referential, investigative, and reasoning interbehavior. Moreover, numbers are not merely symbols of science, but, as plainly appears in the author's chapter on "Number Lore," also symbols of magic.

To the credit of the author be it said that while he implies that number consists of symbols or the "language" of science, his book to a great extent is designed to show that, while mathematicians believed they were dealing with symbols for nothings, infinitesimals, infinites, and imaginary things or talking about such nonexisting entities, actually they were concerned with genuine operations upon relations. The historical records showing how mathematicians turned from the one extreme that mathematics deals with symbols for things toward the other extreme that it consists of nothing but writing symbols must now be emended, it seems, by properly placing number symbols in their various fields of interbehavioral operation. The history of mathematics shows nothing more clearly than that symbols are factors in complex system-building enterprises.

The realization that mathematics is not simply the manipulation of symbols in order to make precise statements, but, rather, the interbehavior with relations by way of discovering, measuring, equating, correlating, and varying them, is well illustrated by the author in a personal experience.

> "I recall my own emotions: I had just been initiated into the mysteries of the complex number. I remember my bewilderment: here were magnitudes patently impossible and yet susceptible of manipulations which lead to concrete results. It was a feeling of dissatisfaction, of restlessness, a desire to fill these illusory creatures, these empty symbols, with substance. Then I was taught to interpret these beings in a concrete geometrical way. There came then an immediate feeling of relief, as though I had solved an enigma, as though a ghost which had been causing me apprehension turned out to be no ghost at all, but a familiar part of my environment" (p. 245).

The achievement of the mentioned interpretation certainly implies an awareness that one is interbehaving with relations in a field of Cartesian coördinates. Probably all mathematics centers about such interbehavior, which, if emphasized, enables one to identify and relate symbol-things, symbolizing behavior, linguistic activity, scientific work and records, as well as technical mathematical operations.

J. R. KANTOR.

Indiana University.

THORPE, L. P. Psychological foundations of personality: a guide for students and teachers. New York: McGraw-Hill, 1938. Pp. xvi+602.

This book is designed by the author " as a textbook in courses wherein the nature of . . . the human personality is given primary consideration. It is also offered as a source of easily accessible information to that host of earnest educators and professional workers in other lines who . . . are anxious to secure a background in this field that will enable them to assist young and old alike in understanding their own natures and consequently in making increasingly better personal and social adjustments."

No original definition of personality is offered, but the indications are that the author favors May's as being the best of the current ones.

Dr. Thorpe has done an excellent job of classifying and summarizing different points of view, especially in the sections on drives, emotions, and the nature of personality traits. It would appear likely, however, that the beginning student might become lost among the welter of studies mentioned and secondary sources cited. An unduly large portion of the book (90 pages) is devoted to intelligence. However, the summary of the literature on the influence of favorable opportunity upon intellectual status is excellent. " Instinct," too, is given a disproportionately extensive treatment, while the cultural factors influencing personality are merely mentioned, and Lewin's extremely important material on personality structure is referred to only in passing.

The author presents a very competent exposition of the nature of the genetic approach to personality, but does not live up to his announced purpose of " following the growing individual from birth through childhood as reflected in home and school, to adolescence and maturity."

A lack of clear thinking is evident in the chapters on " Mechanisms of Personality Disturbance " and " Personality Adjustment and Integration." In discussing personality disorders and their probable causes, attention is called to the social conditions surrounding the child as being of crucial importance, yet mental hygiene is defined as " the *inculcation* (italics reviewer's) in every individual of such habits and attitudes as will logically lead to a happier, richer, and more abundant life."

The style of the book is marred by excessive wordiness. It would seem that Dr. Thorpe never uses one word where two will do, *e.g.* " What a tragedy it is that so many of these well-meaning lesson hearers, whom a recent writer has honored with the epithet ' architects of the wholesome personality development of the nation's children,' should be so unable to detect and interpret the precursors of serious forms of what in legal terms is called ' insanity.' "

The reviewer believes that the book may be used to advantage in a course dealing with personality, provided the instructor is capable of clearing up the confusions which will undoubtedly arise. But it would be unfortunate if " hosts of earnest educators " considered the study of this book sufficient preparation " to assist old and young alike in understanding their own natures."

ROBERT C. CHALLMAN.

Teachers College, Columbia University.

RYLANDER, G. Personality changes after operations on the frontal lobes: a clinical study of 32 cases. London: Humphrey Milford, 1939. Pp. 327.

With the term "personality" being used to include all mental functions, the object of the study reported in this book was primarily to discover the effect of frontal lobe operations upon "the patient's capacity as a member of society." Because of the number of cases and the systematic psychological approach, the monograph is an important one. It is, in fact, partly on account of its general excellence that its flaws demand attention.

During the past decade much attention has been paid to psychological changes following surgical removal of cerebral tissue, and in this field it is a most interesting fact that medical or surgical authors have generally been much more careless about the anatomical than about the psychological problems involved. One could understand a lack of psychological nicety, but it is harder to understand the medical author's lack of attention to the status of the brain tissue left *in situ* or the necessity of determining the true extent of destruction. Removal of a large part of the brain is a dramatic thing, but it should not blind the neuropsychiatrist to everything else that has happened in the brain. Elementary logic demands that if mental changes are to be ascribed to the removal, it must be shown that the removal constitutes the only significant lesion. It is a truism in the neurosurgical clinic that the extent of destruction after removal of a brain tumor is almost impossible to determine because of the displacement of "normal" tissue by the growing neoplasm and because of possible pre- and postoperative vascular change in the rest of the brain; but this fact receives little attention in published case reports.

Of the thirty-two cases reported, one was a case of abscess; the rest, cases of tumor. The author, a neurologist, has made no real analysis of the anatomical data nor has he considered the possibility of pathological destruction in the rest of the brain. Infiltrating tumors are not distinguished from meningeal tumors; yet recurrence of the infiltrating tumor is highly probable, and postoperative symptoms cannot safely be ascribed to the surgical removal alone. In several cases the tumor removal was known to be incomplete and was doubtfully complete in others. In case 2, again, the statement is made that "a large artery, probably the left anterior cerebral artery, was ruptured directly above its origin in the internal carotid artery"; yet this case is included with the rest, without noting that there was probably an extensive necrosis in addition to the surgical removal. What should be the special significance of the case of brain operation in the study of localization of function? It should be in knowing the definite extent of the lesion, but this is just what we do not know after such brain operations. Rylander is by no means alone in his uncritical attitude toward the anatomical data, but, with an increasing number of cases for study, there is no longer much excuse for lumping everything together in one category of "brain operations."

The psychological aspects of the work are good. A wide battery of tests was used systematically, including a translation of the unrevised

Stanford-Binet, a sorting test, learning of nonsense syllables, an excellent test of arranging a series of pictures to tell a story ("The Apple Thief"), a test of fatigue, and others, although no speeded performance tests were employed.

The author should be congratulated on his use of a matched normal control group. Although he does not analyze them extensively, the results are of considerable interest. The clinical group, for example, with considerable impairment of immediate memory, was inferior to the normal group in the retention of word-associations and in nonsense syllable learning, but not in the *learning* of word-associations or in syllable and digit memory span. Again, the clinical group was not inferior in vocabulary scores, but was inferior in the ability to distinguish the meanings of abstract words. Here and elsewhere, the author's conclusions are group conclusions. Since he reports cases in which little or no change was found after operation, it is evident that the group findings do not apply to the individual case, although the wording is general and does not mention the exceptions: "The results obtained in this investigation bear witness to the fact that marked changes occur in all the domains of the higher mental life . . ." (p. 295); and, "Mental changes occur after excision of parts of the frontal lobes" (p. 300).

As a group (but again, not in all cases) the patients operated upon showed most of the frontal lobe signs of the literature, with thirty of the thirty-two having lasting mental changes of wide variability in kind and degree. The book therefore appears to give an excellent account of the variability of symptoms to be looked for in frontal lobe lesions. If the title had been modified to make it clear that the study was really concerned with the mental changes following operation *for frontal lobe tumor,* without implying that removal of a healthy frontal pole would necessarily have the same effect, the conclusions might have been accepted with little reserve. The rapidly infiltrating tumors were purposely excluded, and the monograph gives no picture of the situation in these cases. Otherwise, it should be a valuable corrective to accounts based on a more limited number of cases or on less thorough psychological study.

D. O. HEBB.

Queen's University.

VIALLE, L. Défense de la vie. Paris: Félix Alcan, 1938. Pp. 166.

This little book, a literary delight for those who love clarity and felicity of style, is written by a professor of philosophy in a French lycée, where, in the final year, equivalent to the sophomore year of college with us, the student is occupied primarily with taking a course which gives him philosophy, psychology, ethics—all in one. Louis Vialle is a contributor to French journals of psychology, besides writing books in the field of psychology. But his psychology is not experimental; it is, rather, philosophical, or in this book, almost religious. In fact, one has the feeling of reading a new chapter in James's study of the varieties of religious experience. The title of the book, by itself not very revealing, indicates the author's purpose of seeking what may promise to defend life against pessimism over its pain and transitoriness, its solitude and frustrations, which reason, here depicted almost as an enemy of life, reveals. Vialle, then, is looking for a way of salvation. Since it is reason primarily that shows man that he is doomed to go, and go alone, as he came, life turns to faith for its defense, and particularly to faith in God. It is surprising to see to what degree Kierkegaard, the Danish theologian of the last century, whose rediscovered teachings have influenced so tremendously contemporary German philosophy (Heidegger and Jaspers) and German or Swiss theology (Barth and Brunner), has also influenced this French thinker. Not that Vialle accepts Kierkegaard's antirationalism totally; but he tries sympathetically to understand the psychological needs from which it springs and the psychological ends which it serves. Of course, Bergson had prepared French thinkers for the essentially un-French view (notwithstanding Pascal) of reason as having value only as a servant of life; and Vialle is strongly influenced by Bergson, especially by the latter-day Bergson who wrote *Les deux sources de la morale et de la religion.* Still, it is indicative of a new wind that is blowing in France that anti-intellectualistic faith should be treated as sympathetically as it is here by a French thinker. It must be added, however, that most of Vialle's conclusions are introduced by a "peut-être," which shows that the whisperings of scepticism, so typical of French thought from Montaigne to Anatole France, could not be silenced.

Besides dwelling upon the pessimism derived from the spectacle of man's transitoriness, Vialle makes much of the human tragedy of solitude. We live and die alone. Now, religion, particularly the mystic's, attempts to overcome this intolerable sense of cosmic and social loneliness by searching for, and attaining—if the mystic is to be believed—union with God. In this ecstatic experience, often impugned because of the mystic's speechlessness after his return, we must not, according to Vialle, expect the characteristics of knowledge, which, upon an analysis that owes much to Schopenhauer and Bergson, is shown always to involve a subject-object relationship in which there remains an unbridged distance between the solitary knower and the known. The mystic's way would indeed be an enchanting way of salvation for life, paradoxical though it may seem, if one could but have the faith necessary for it. Vialle knows full well that a faith known to be an illusion will not save; but, he asks, does not life show its resourcefulness and subtlety in reaching out for this faith which will accept neither annihilation nor loneliness, no matter what reason may say? We are here reminded of Nietzsche's great struggle of loyalty between reason and life and his final assurance to life that he always loved her most. Vialle's persistent scepticism prevents him from going as far as Nietzsche. How far he goes precisely is not made quite clear, but this much is certain: for him also, reason has to serve life whose ends and needs are supreme. Illusions, even when beneficent, must be rejected, but only because, when known to be such, they would not serve life as well as reality.

This book, then, in conclusion, is an interesting and valuable book, not so much for its psychology in the stricter sense, but because it is revelatory of what unexpected winds of doctrine are blowing in the France of today.

CORNELIUS KRUSÉ.

Wesleyan University.

FINK, A. E. Causes of crime: biological theories in the United States, 1800–1915. Philadelphia: Univ. Pennsylvania Press, 1938. Pp. xi+ 309.

This book summarizes the various attempts on the part of American physicians, prison administrators, and others during the Nineteenth Century to find in biological factors the explanation of criminal behavior. The survey begins with the influence of phrenological doctrines on American theories of the causes of crime and ends with the unsuccessful attempts to establish feeblemindedness as *the* causal factor. The author's aim is to provide an account of hitherto neglected American contributions to the now discredited theories of crime causation of European origin. The author has supplied in painstaking detail the references to American contributions which he complains are lacking in the the American textbooks on criminology. (His bibliography includes more than 800 titles; the majority of the references are to journal articles.) The reviewer is inclined to disagree with the author as to the reason for the previous neglect of this field on the part of students of criminology and believes that the material is not of a sufficiently consequential nature to justify such detailed consideration.

The history of the pseudoscientific study of crime causation in America begins with phrenology in the hands of Caldwell, who defended the thesis, based on the familiar doctrine of Gall and Spurzheim, that the criminal nature could be understood only in terms of an analysis of the powers indicated by the conformation of the skull.

Of far greater influence than the phrenological theory were the various theories concerning the relationship of insanity and crime. Controversy centered around the problem of who was insane and who was criminal in 1838 as it does a hundred years later. In 1838 Isaac Ray, of the Harvard Medical School, distinguished between the act of an insane man and the act of a criminal on the basis of intent. This distinction still constitutes the basis for medicolegal controversies over the philosophies of treatment *versus* punishment.

Then follow chapters on moral insanity, which is characterized by "irresistible impulses" to commit motiveless antisocial acts; the effect of alcohol and drugs in the production of a mental disease which activates the individual to the production of crime; the evidence for the existence of a criminal brain, and other anatomical deviations; the inheritance of criminality and the sterilization of criminals; and, finally, an evaluation of the rôle of feeblemindedness in crime causation with the theory of moral imbecility, which is logically related to the theory of moral insanity.

These chapters are all meticulously documented and contain frequent quotations from widely diverse sources in medical journals, proceedings of medical and prison associations, and medical jurisprudence.

An admirable final chapter serves to draw together the many scattered threads of theories of biological determinism and analyzes the reasons for the uncritical acceptance in America of current European theories and our failure to originate theories of crime causation. As the author points out, " if any one fact stands out concerning the explanations which flourished during the period of a century or more in America, it was the fact that there was no one school, no one theory, of crime causation which was indigenous to America. This is not to say that there was a lack of theories, but rather that the abundance of theories was taken over from the European scene." Finally, the author points out that the reason for the evaluation of these contributions of American physicians and others to the errors they shared with their time is that " today's science could not have been built save for the science of yesterday," and that " with the aid of these developments, a more scientifically adequate understanding of crime causation was possible."

MAUD A. MERRILL.

Stanford University.

KELLER, W. Der Sinnbegriff als Kategorie der Geisteswissenschaften. (Teil 1.) München: Ernst Reinhardt, 1937. Pp. 176.

There is probably no psychologist, regardless of school, who is not forced occasionally to face the problem of the relationship of " meaning " to psychological subject matter and method. A work which pretends to define the place of meaning in the " sciences of mind " should consequently have a claim upon the interest of the psychologist. The first volume of such a work lies before us. It must be said immediately that the psychological reader will be disappointed. He will find the work to be that of a philosopher of the most abstract leanings, whose understanding of (and even interest in) actual research in the mental sciences appears to be slight.

Since the work is not concerned primarily with psychological matters in particular, a detailed résumé of the contents is unnecessary in this connection. Certain conceptions run like *idées fixes* through the work. The author develops at great length, for example, his view that subject matter no more determines method than does method determine subject matter, both being determined by a superordinate idea (*Idee*). We also learn that *indefiniteness* is not a positive characteristic of subject matter itself, but is rather a mark of inadequacy of method!

The chapter dealing with " meaning, understanding, and insight " has the closest bearing upon psychological problems. At the present time a satisfactory presentation of such a topic would necessitate some coherent historical review of the literature, but this we do not find. The significant contributions of Dilthey, Spranger, Rickert, and others are hardly more suggested, and the name of Jaspers, unless we are mistaken, does not appear at all. The chapter appears to shed no further light on a difficult set of problems.

In the work as a whole, there is little trace of the utter clarity of a Rickert, the brilliant alertness of a Scheler, or the humanized erudition of a Dilthey. Instead, we are offered, far too frequently, such pronouncements as these: "Das Sein, das nur als Sinn von irgendwie aufweisbarem Bestimmten möglich ist, ist selber die Seinsmöglichkeit des Soseienden in seinem Sosein" (p. 28); "Faktische Existenz aber ist dann die Möglichkeit der Möglishkeit der Möglichkeit [*sic!*], denn sie ist der Ort des realen Seinkönnens des grundsätzlich schon möglichen Vorweghabens von Möglichkeit" (p. 47).

A reading of this volume suggests more than anything else the paradoxical fact that it is possible to deal so meaninglessly with meaning and so unintelligibly with understanding.

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CHARLES WARREN FOX.

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NOTES AND NEWS

President Day, of Cornell University, announces that Dean R. M. Ogden, who has heretofore retained a position in the department of education, has now taken a professorship in the department of psychology and will give this year at least 1 course in psychology. President Day also announces that Professor H. S. Liddell and Assistant Professor O. D. Anderson have been transferred from the Cornell Medical School, Ithaca Division, which has been discontinued, to the department of psychology. Their experimental researches in psychobiology, particularly in conditioned reflexes, endocrinology, and experimental neurosis, will be continued. Their new laboratory, the Cornell Behavior Farm, is now combined with the laboratory in Morrill Hall, bringing together under one administration all of the personnel and facilities for research in psychobiology at Cornell.

A **portrait** of Dr. Edward Lee Thorndike, professor of educational psychology and director of the Division of Psychology of the Institute of Educational Research at Teachers College, Columbia University, has been presented to Wesleyan University, from which he graduated in 1895. The portrait, which was painted by Howard L. Hildebrandt, was placed on public view on June 18 in the Honors College of the university. —*Science.*

The honorary degree of doctor of music was conferred at the commencement exercises of the Chicago Musical College on Dr. Carl E. Seashore, research professor of psychology and emeritus dean in the State University of Iowa, in recognition of "his contributions to the science of music."—*Science.*

M. Henri Piéron, professor at the Collège de France and director of the Institute of Psychology of the University of Paris, was elected at the Liège meeting president of the French Association for the Advancement of Science.—*Science.*

Dr. Edwin E. Ghiselli, formerly instructor in psychology at the University of Maryland, has been appointed assistant professor of psychology at the University of California.

Dr. Matthew N. Chappell, who is on the staff of Columbia University and conducts experimental and clinical research in the field of emotions, has recently joined the staff of the Psychological Service Center in New York under the auspices of the Psychological Corporation.

Dr. Wallace H. Wulfeck, who has been conducting an extensive program of research on interstate truck drivers for the Division of Industrial Hygiene of the United States Public Health Service, is now connected with the Psychological Corporation, sharing his time between work in market research and clinical counseling.

The honorary degree of doctor of science was conferred on Dr. C. E. Ferree, director of the Research Laboratory of Physiological Optics, Baltimore, by Ohio Wesleyan University at its commencement on June 12.

Plans have been made to hold the Twelfth International Congress of Psychology in Edinburgh, Scotland, from July 22 to 27, 1940. On account of the war, no further details are being published at present.

Among those receiving awards of the Social Science Research Council for the academic year 1939–1940 were the following:

Albert A. Campbell, Ph.D. Stanford University, instructor in psychology, Northwestern University, for advanced training in anthropology and in social psychology in England.

Raymond F. Sletto, Ph.D. University of Minnesota, assistant professor of sociology, University of Minnesota, for advanced training in the United States in mathematics and social psychology.

Lucien M. Hanks, instructor in psychology, University of Illinois, for a study of deviants among the Blackfoot Indians.

Elaine F. Kinder, research psychologist, Letchworth Village, for a follow-up study of social adjustment of 100 defective girls.

Theodore M. Newcomb, Department of Psychology, Bennington College, for a study of personality concomitants of attitude changes of undergraduates over a period of years in one college.

Watson O'Dell Pierce, consulting psychologist, Columbia Grammar School, for an analysis of the specific vocational interests and job satisfaction of certain American and Scottish occupational groups.

Saul Rosenzweig, psychologist, Worcester State Hospital, for the validation and standardization of a multiple choice questionnaire for types of reaction to frustration.

Ross Stagner, assistant professor of psychology, University of Akron, for a correlational analysis of nationalistic opinions.

Dorothy Rose Disher, assistant professor of psychology, Florida State College for Women, for a study of sectional differences in masculinity-femininity reactions.

Evelyn Raskin, instructor in psychology, Randolph-Macon Woman's College, for study of the effect of nursery school environment on changes in intelligence test scores of underprivileged Negro children.

ATTENTION is called to the new individual subscription rate to the *Psychological Record.* This rate is $1.00 per year of 1 volume to individuals. The regular rate for institutions and libraries remains at $4.00 per year. The *Psychological Record* was started in 1937 by Drs. J. R. Kantor and C. M. Louttit to afford psychologists a medium for immediate publication. While the pressure on psychological journals has lessened in the past 3 years, the *Record* still seems to be serving a useful purpose. The journal is a coöperative arrangement and depends upon subscriptions to cover the difference between charges to authors and manufacturing costs. At the present time the *Record* is considerably in debt, and, if it is to continue beyond the present volume, the number of subscriptions must be materially increased. Three hundred individual subscriptions would come very close to balancing the budget.

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Psychological Bulletin

A CRITICAL REVIEW OF CURRENT APPROACHES TO AFFECTIVITY

BY WILLIAM A. HUNT

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The purpose of the present article is twofold: first, to make a survey of the recent work dealing with pleasantness and unpleasantness; second, to attempt to fit these independent and occasionally antithetical studies within a common framework.

There will be no attempt to include the entire field of emotion. For our purpose, the affective states will be given their narrower definition of pleasantness and unpleasantness (61). Representative studies will be treated, but there is no intention of presenting a complete and exhaustive bibliography. Only the work of the last few years will be considered. The earlier material is easily available in the thorough studies of Beebe-Center (2) and Ruckmick (56). The history of the field also has been treated recently by Gardiner, Metcalf, and Beebe-Center (17).

In his *Experimental psychology* Woodworth concludes his treatment of pleasantness and unpleasantness with the following commonsense statement: "A general conclusion, not exactly forced by the evidence from introspective studies of feeling, but at least rendered attractive, is that feelings are reactive attitudes of the organism. Pleasantness and unpleasantness correspond to the attitudes of acceptance and rejection . . ." (62). It would seem fairly safe to assume that most psychologists today would agree that the concepts, pleasantness and unpleasantness, as used in psychology tacitly refer to general attitudes of acceptance and rejection and that the field of affectivity covers the investigation of these attitudes in their development and operation.

Despite the common agreement on this general theoretical background, most of the experimental and theoretical studies have been limited to narrow assumptions of a more specific nature. Thus, we have the treatment of affectivity as an end-organ process, as a correlate of thalamic activity, as the accompaniment of specialized cerebral processes, etc. None of these is necessarily hostile to the others or to the broader treatment of affectivity as a general attitude of acceptance or rejection, but their apparent independence often creates a confusing impression of chaos within the field; and the lack of any broad integration frequently results in a loss of perspective.

This tendency has been encouraged by the close association of affectivity with the introspective method and its structural, sensory bias. The recent unpopularity of this approach has resulted in the relative neglect of affectivity and an attendant feeling that research in this field is contrary to, or at least lies outside of, the main currents of contemporary psychological investigation. The present article will attempt to reorient the recent studies within a wider framework, thus dispelling some of the present appearance of confusion in the field and indicating its relation to other current trends in psychology.

For the purpose of our survey a fourfold classification will be useful. We shall classify the work according to whether it deals with pleasantness and unpleasantness as judgments, with pleasantness and unpleasantness as conscious contents, with the physiology of the affective states, or with what objects are pleasant or unpleasant. These 4 approaches represent the main trends in the subject today. Occasional odd studies may be difficult to place, but, in general, our 4 categories will prove valid. No mention will be made of the work on the influence of affect on memory and learning. In the past this his been treated largely as a field by itself and has been reviewed as such, *i.e.* Gilbert (**20**).

PLEASANTNESS AND UNPLEASANTNESS AS JUDGMENTS

Recognition of the fact that meaning plays a rôle in affectivity is not new, but of late the further understanding and clarification of this rôle has given it new importance. This has developed with the increasing realization that pleasantness and unpleasantness are concepts that refer, not to some specific phenomenon, but to a wide range of related events, and that their usage by a subject follows the usual processes of judgment.

Carr's (7) judgmental theory of affectivity has played an important part in this development. Unfortunately, the fact that it was first stated in an introductory textbook kept it from being very widely known at first. According to the judgmental theory, affectivity is wholly a matter of meaning. Pleasantness and unpleasantness are judgments that the organism renders in the light of its normal reaction tendencies toward the stimulus object. If such reaction tendencies are positive, the object is called *pleasant,* and the recognition of the existence of these positive reactions is called *pleasantness.* If negative, the object is called *unpleasant,* and "unpleasantness" is said to exist. ". . . Pleasantness and unpleasantness are names for the two distinctive *meanings* which objects may acquire in virtue of the type of adaptive response which they normally elicit." "The affective aspect of an object is a product of a certain type of cognitive reaction to that object, and it has no existence apart from that reaction; the unpleasantness of an object is not a given characteristic of that object whose existence is subsequently noted and reported" (p. 298). Pleasantness and unpleasantness are thus clearly identified as meanings based on the subjects' habitual reaction tendencies toward the object. Nor is the actual arousal of the response in each specific situation essential to the delivery of the affective judgment, *i.e.* one "likes" ice cream even when, temporarily, he has eaten too much of it.

Peters (**50**) has recently restated the theory, related it in thorough fashion to previous work, and presented an elaborate defense of it. McGeoch (**41**) has called attention to the importance of the genetic aspects of Carr's hypothesis and hence to the relation between affectivity and learning.

In its frank espousal of pleasantness and unpleasantness as "meanings" and its clear and specific description of how such "meanings" are established, the judgmental theory is shedding light upon one of the most perplexing and persistent problems in affectivity. Moreover, if we may evaluate it in terms of our fourfold classification, the theory is reinforced by the constantly increasing body of current research upon the nature of affectivity as a process of judgment. Those studies which deal with pleasantness and unpleasantness as concepts covering a wide range of reference, and which establish their dependence upon the related psychological phenomena of learning, memory, and judgment, all offer at least partial support to the judgmental theory. This will become clear when we survey such studies at greater length below.

The next 2 lines of research, those upon the conscious contents attendant in affectivity and upon the physiology of affection, are not so happy in their relation to the judgmental theory. The theory denies the existence of any uniform conscious content. It also denies the existence of any behavioral uniformities in affection. The response tendencies are held to be "as manifold as the possibilities of the human repertory of behavior will allow" (**50**).

There is no reason, of course, why the judgmental theory could not incorporate both a uniform conscious content and uniformities of behavior within its broad outlines, although to do so would sacrifice much of the uniqueness of its present formulation. As it stands, however, the theory states that pleasantness and unpleasantness are judgments rendered merely on the basis of normal reaction tendencies of a positive or negative nature and not on the basis of any uniform regularity of behavior appearing in such response tendencies. Any evidence of the existence of such uniformities of response must be interpreted as evidence against the theory in its present form.

The fourth type of problem, what objects are pleasant and unpleasant, is avoided by the judgmental theory. Affection is no longer directly determined by objects, but by the response tendencies aroused by these objects. The work on preference and choice would still exist, but would presumably be delegated, not to affectivity, but to motivation. This shift in stress does not seem important.

As Peters (**50**) points out, the judgmental theory is open to experimental verification. In a series of 4 studies (**51**, **52**, **53**, **54**) he has shown that affective value may be influenced by experimentally induced attitudes of acceptance or rejection. He had his subjects rate 10 Japanese words for affective value. They were then put in a learning situation where the 10 words were exhibited successively, and the subject had to learn to respond to 5 of them positively, by pronouncing each when it was shown, and to react negatively to the other 5 by not pronouncing them. Failure, indicated by the ringing of a loud bell, consisted of failing to pronounce a word which should have been pronounced or of pronouncing one which should not have been pronounced. After this task had been learned to a criterion of 5 errorless trials, the words were again rated for affective value, and it was found that the words to which the observer had learned to react positively by pronouncing them increased in affective value, while those to which negative reactions had been learned decreased in affective value. He duplicated these results, demonstrated the validity of his rank order scale values, showed that the affective shift was absolute rather than relative, and found that the effect persisted over the interval of 1 week, although it diminished in strength during this time.

Peters' work leaves no doubt that changes in affective value may be set up under the conditions of his experiment. As he himself points out, however, the factors entering into the situation are too complex to admit of its being interpreted as an *experimentum crucis.*

As an example of the complexity of the factors involved, Flugel (**16**) has recently mentioned some unpublished work of Pan (**49**), in which an attempt was made to influence the affective value of a stimulus by asking the observer deliberately to change his attitude toward it—say, from aversion to seeking. The feeling tone frequently could be altered in this fashion, but there was usually some shift in cognitive content or emphasis which seemed to indicate that the affective shift was not the immediate result of the changed attitude but, rather, the concentration of attention upon some new aspect of the stimulus.

Until such possibilities can be controlled, Peters' work cannot be considered as doing more than offering partial support to the judgmental theory. At present it leaves unanswered 4 important questions: 1. Are pleasantness and unpleasantness specifically and indubitably determined by the reaction tendencies set up or are they determined by some other element of the complex learning situation? 2. Are such reaction tendencies the only determinants of affect? 3. If they are not the sole determinants, what is their relative importance? 4. Do the positive and negative response tendencies contain any uniformities of behavior, and are they accompanied by any uniformities of consciousness? The validation of the judgmental theory as formulated by Carr and Peters will necessitate a final answer to these questions.

A somewhat similar hypothesis has been proposed independently by Hunt (**28**). In an attempt to explain the conflicting results often found in the field of affection he proposed that the terms "pleasantness" and "unpleasantness" be viewed as broad, generic concepts covering a multitude of related, but by no means identical, phenomena. He suggested that the development of these concepts be considered as a process of verbal conditioning with the words *pleasant* and *unpleasant* as verbal responses being originally attached to some bodily experience specific to affection (in his example, bright and dull pressure) and becoming attached in the course of experience to other elements of the affective situation. Once these verbal responses were established, they might even be used independently in the thought processes without reference to any immediate bodily involvement. This differs from the judgmental theory in its use of "verbal conditioning" instead of "judgment," and especially in its assumption of some specific conscious content as the original reference of the term.

The development of many of our hedonic and emotional terms as concepts with a wide range of reference has been stressed by Munn (**43**). It is brought out clearly in the study of naïve definitions within the field (**30**). These show that the words *pleasant* and *unpleasant* may refer to objects and to relationships in the environmental situation, as well as to purely sensory experience.

Ruckmick (**56**) has rejected such a view (and by implication the multiple reference inherent in the judgmental theory itself) as "nominalism," and calls for a univocal reference for the terms. He seems to have misunderstood the argument. We are dealing, not with the *systematic* question of what would be the best definition for our hedonic terms, but with the *empirical* question of how they actually are defined in current usage. Past and present work in the field can only be understood after a thorough investigation of the various meanings of our terms. Once this is done, we may, if we wish, proceed in the future to establish a univocal definition by arbitrarily limiting the reference of the concept to a specific one of its various meanings. This is a problem, not of nominalism *versus* realism, but of simple definition, and it can be solved as such.

The fact that the hedonic situation involves a judgment on the part of the observer is confirmed by the increasing number of studies which point out similarities between the results of hedonic experiments and those of psychophysics. Hunt and Volkmann (**32**) have demonstrated that it is possible to duplicate with the affective judgment certain shifts in scale value (involving anchoring effects with absolute scales), previously noted by Volkmann (**60**) in psychophysical experiments on the judgment of the inclination of lines. They suggest the operation of "general principles of judgment in both fields." Dashiell (**13**), in a study of the affective value-distance as a determinant of esthetic judgment-times, also has found results analogous to those in psychophysics. Cohen (**11**) demonstrated that affective judgments showed a relativity due to the scale used. While these experiments do not indicate that affective value is created solely by the processes of judgment *per se,* they do point to the importance of such processes in the final result.

McGeoch (**41**) has stressed the genetic aspect of the judgmental theory and indicated the importance of learning in it. Little work, however, has actually been done on this. E. B. Hunt (**24**) has studied the development of hedonic terms in children's vocabularies, and found that the words *pleasant* and *unpleasant* appear relatively late and are preceded by such words as *good, bad, like,* and *love.* Further work of this kind should aid in understanding the genesis of the hedonic response. In a study of the variability of the affective judgment Hunt and Flannery (**31**) noticed variability behaving very much as does error in the learning experiment. Whatever final conclusions one may reach from all this work, it certainly demonstrates that the processes of learning and judgment play an important rôle in the final determination of the affective response.

PLEASANTNESS AND UNPLEASANTNESS AS CONSCIOUS CONTENTS

Let us look at the phenomenological description of affectivity. The most recent trend here was begun with the work of Nafe in 1924 (**44**). Nafe criticized the earlier work on the grounds that the stimuli were so intense as to distract the observers from their primary task of introspection. They were feeling rather than observing feeling. To avoid this, he used less intense stimuli and found his subjects describing 2 different experiences, both apparently sensory in nature. One, which appeared in pleasant situations, was a bright, lively, effervescent feeling resembling a bright pressure or tickle. It was diffuse and volumic. The other, appearing in unpleasant situations, was a dull, constricted, leaden feeling. Nafe calls these " bright pressure " and " dull pressure " and considers them to be touch qualities of a sensory sort. Later work in the Cornell laboratory by Horiguchi localized dull pressure in the abdomen and bright pressure less definitely in the upper body regions. This study is quoted by Hoisington (**23**), but has never been published. Nafe has accepted this localization: " Pleasantness, as a psychological experience, consists of a pattern of discrete bright points of experience in the general nature of a thrill but usually is much less intensive. It is vaguely localized about the upper part of the body. Unpleasantness is similar but characteristically duller, heavier, more of the pressure type of experience, and is localized toward the abdomen or the lower part of the body " (**46**).

There are 2 aspects of Nafe's work that should be kept separate, but unfortunately are not. The first aspect is the reporting by Nafe's observers of common experiential elements in the affective situation and their general description under the constant language difficulties attending phenomenological observation. This discovery of 2 types of experiential content in the affective situation is the important contribution of Nafe's experiment. The second aspect is Nafe's use of the names "bright pressure" and "dull pressure" to designate these experiences and his statement that they are sensory in nature. This step entails the association of these experiences with the pressure sensations, their relation to the Titchenerian touch pyramid, and their inclusion in the whole Titchenerian systematic context. This is an additional step and involves further inference beyond the original phenomenological data.

Much of the protest against Nafe's work seems to stem from this second, less vital, aspect of his work, rather than from the first. As Hoisington points out (23) and other work shows (29), observers often object to these names, bright and dull pressure, but are unable to suggest anything better. The phrases "light, expansive feeling in the chest" and "dull, contracted feeling in the abdomen" have been used with some success (29). If we are not to lose sight of the true significance of Nafe's work, we must fix our attention on the phenomenological data rather than on the issues appearing due to terminological difficulties and "systematic" interpretation. These last are important, but their importance is secondary.

Later checks upon Nafe's work have yielded varying results. Young repeated Nafe's experiment with 3 observers (63). Only 1, who had previously served in Nafe's investigation, reported bright and dull pressures. Young concluded that these pressures are a product of laboratory training and systematic bias. This matter of laboratory prejudice and suggestion is tricky. Certainly, one is just as justified in assuming that negative results are the result of negative bias as in assuming that positive results are the result of positive bias. Unfortunately, the argument is always assumed to apply only to the other fellow.

As Nafe (45) points out, these pressures are not attention demanding. The observers must be set for them. Such instruction or set, however, must be accomplished without influencing the observer through any form of positive suggestion, if Nafe's opponents are to be satisfied. Hunt (25) attempted to fulfill these conditions by instructing his subjects concerning bright and dull pressure. They were then asked to note whether or not a series of color stimuli produced these pressures. No mention was made at this time of pleasantness or unpleasantness. At a later date the subjects were recalled and asked to judge the affective values of the colors. Correlating the 2 series of judgments gave coefficients that ranged from +.72 to +.99 and approached unity when corrected for the attenuation produced by affective variability.

Hunt took a conservative view of this as expressing co-relationship rather than identity and concluded that pleasantness was accompanied by bright pressure and unpleasantness by dull pressure. These pressures are not identified as affectivity, but merely as one element in the total affective response. As regards bias, it might be mentioned that the majority of Hunt's subjects were naïve observers without previous contact with psychology. While the atmosphere of the Harvard laboratory was favorable to the introspective approach, there was much scepticism concerning bright and dull pressure, and Hunt himself undertook the experiment, believing that its results would refute Nafe's claims. The work was repeated in emotional situations and bright pressure was found to accompany pleasant emotions, while dull pressure accompanied the unpleasant ones (26).

While spontaneous localizations had been reported in the previous studies, a final experiment was devoted to this alone and confirmed Horiguchi's localization of bright pressure in the upper regions of the torso and dull pressure in the region of the abdomen (27). During this last experiment Hunt found that his observers were able, before being instructed for bright and dull pressure, to report the presence of ordinary localizable pressure sensations in the body cavity. With pleasantness these were located in the upper chest, while with unpleasantness they were placed in the abdomen. Hunt concluded that these pressures were specific components of a general complex of sensations corresponding to " bright " and " dull " pressure.

A direct attack has been made on these results by Converse (12). Duplicating Hunt's conditions with groups of 4, 5, and 6 observers, she finds average correlations for her groups of +.30, +.36, and +.31. These correlations include 3 of 0, which are all cases in which the observer failed to report any pressures at all. The propriety of calling these ' zero correlations ' hinges around the sincerity of the observers. In the previous work subjects were occasionally found who claimed to be unable to make pressure judgments (27), but always after persevering in the task they finally succeeded. This persevering was not accompanied by any " training," the only further instruction being to " keep on trying." One cannot help but wonder how hard Converse's observers tried, and whether they were encouraged to keep on trying, or whether their claimed inability to make judgments was accepted immediately without any further question. If this last were true, it might well indicate a negative laboratory bias just as vital as any positive bias in the work of Nafe and Hunt.

With another group of observers, Converse used tones as stimuli. The results here are admittedly no better than chance, but Hunt has previously noted the confusion attendant upon making pressure judgments with auditory stimuli, mentioning the " tendency to confusion in judgments of bright and dull pressures where local tactual pressures arose from the stimulation . . . " (25). With this in mind one may well question the validity of this section of the work. A final criticism that may be used is that Converse made no effort to correct her correlations for the attenuation due to affective variability. This further duplication of Hunt's technique would be only fair, and its omission is regrettable.

One can concentrate on the failure of Converse to duplicate the previous high correlations, or one can stress the fact that her correlations were, in general, positive though low. Keeping in mind the criticisms mentioned above, the difficulty of phenomenological description, and the undoubted necessity for care and effort in the introspections involved, it seems at least a partial vindication of Nafe's work.

A questionnaire was submitted by Hunt (29) to a large group of introductory psychology students unacquainted with the field of affectivity. The questionnaire began, " It has been said that . . . ," and stated the association between bright and dull pressure and affectivity. The students were asked to judge the statement in the light of their own experience and indicate whether they considered it to be true, probable, improbable, or not true. The questionnaire was repeated on another large group with " light, expansive feeling in the chest " and " heavy, contracted feeling in the pit of the stomach " substituted for bright and dull pressure. In both cases the results preponderantly favored the acceptance of bright and dull pressure as related to pleasantness and unpleasantness.

Ruckmick (55) rightly has pointed out the suggestion inherent in the words " it has been said " and has repeated the questionnaire with proper controls. Again the results, in general, favor Nafe's doctrine, although Ruckmick is cautious in accepting them.

In a different type of approach, Young (64) took 43 of the terms Hunt (25) used as descriptive of bright and dull pressure and had them rated by 228 observers as to whether or not they suggested pleasantness and unpleasantness. As one would expect, the words describing bright pressure are pleasant and those describing dull pressure are unpleasant. Young uses this to refute Nafe by saying that the pleasant associations of the words used in describing bright pressure suggest its connection with pleasantness, and that the unpleasantness of the terms describing dull pressure operate by suggestion to establish its correlation with unpleasantness.

This experiment is capable of a twofold interpretation and may be used to support Nafe as well as refute him. If the pressures are related to affectivity, then what is more natural than that the terms used to describe the pressures should achieve an hedonic tone through association with them? In other words, it is as logical to interpret the work as showing that the relation of bright and dull pressure to affectivity suggests the hedonic tone of the descriptive terms in question as it is to assume that the hedonic tone of the descriptive terms suggests the relation of the pressures to affectivity. The experiment becomes convincing only if one approaches it with a definite set of preconceptions. In itself it is exceedingly ambiguous. When Young says that it would be unfortunate if his discussion obscured genuine problems, and wishes that Nafe's statements could be tested by a method not involving verbal suggestion, he overlooks that part of Hunt's experiment in which observers, without mention of bright or dull pressure or affectivity, were asked to report merely upon any "internal pressures" that might be present. Such pressures were reported, with pleasant colors being accompanied by pressures higher up in the body cavity than those accompanying unpleasant colors. Four of the subjects who had previously observed bright and dull pressures reported these simple pressures as different. The other 3 subjects were "new" and "uncontaminated." These results indicate the presence of some sensory complex with elements of pressure in the usual affective situation.

Another oblique attack was attempted by Converse (12), who succeeded in showing a high correlation, with 4 subjects, between judgments of colors by the paired terms pleasant-unpleasant and personal-impersonal. She concludes that this demonstrates the relative futility of the method of correlation in such a situation. "Ask a set of O's, with the suasion that goes with the atmosphere of an experiment, to judge a set of stimuli by means of two sets of terms, each containing antithetical terms, and we venture to predict that more often than not high correlations will be found from such procedure." Yet this is exactly what she did *not* find in her own work on bright and dull pressure. Moreover, she overlooks the fact that if pure chance were working one would not expect the relationship to be in a constant direction. A more logical interpretation of her work would suggest some possible significant relation between the personal-impersonal continuum and the affective one.

While this survey of the recent experimental work indicates that the case for bright and dull pressure is far from clear, if one keeps in mind the difficulty of the introspections involved, the difficulties of phenomenological description in general, and the undoubted hostility in some quarters toward any Titchenerian concept, the evidence for it would seem to be a bit better than that presented against it. Even in the 2 most bitter attacks, those of Converse and Ruckmick, the results show a balance on the positive side. It would seem only fair to conclude, not that bright and dull pressures exist as definite touch qualities and can be identified as pleasantness and unpleasantness, but that there seems to be an identifiable conscious content with a bodily reference as one of the elements of the affective situation. The verification of this experience, its adequate description, and its possible reference to some bodily process offers an experimental problem whose answer should tell us much about affectivity.

This conscious content may be regarded as the correlate of a specific bodily adjustment and thus be interpreted as rather indirectly confirming the view that pleasantness and unpleasantness involve specific bodily adjustments, despite the opinion of Carr and Peters that such constant processes are not found in the hedonic response.

Nafe's position is not clear on this point. He stresses the sensory nature of bright and dull pressure, but does not state the manner in which these sensations are aroused. Keeping in mind his identification of these pressures as resembling true pressures or touch qualities, and remembering the way in which he and Wagoner have based the temperature qualities upon the reactive possibilities of the smooth muscle of the blood vessels (47), one seems safe in assuming that he would not be unsympathetic to the theory that pleasantness and unpleasantness are bodily attitudes which result in the stimulation of end-organs for bright and dull pressure.

Beebe-Center explicitly adopts such a view in his "heuristic hypothesis." His main interest is in the sensory nature of hedonic tone, but he accepts the view that this sensory process is aroused internally by postural stimuli when he says: "The process in question is invariably proprioceptive. It is aroused by external stimuli—*e.g.* visual stimuli—indirectly through the muscular adjustments which they bring about" (2).

Hunt (28) also takes the view that bright and dull pressure result from bodily attitudes, although he rejects the sensory interpretation in favor of one in which the pressures are viewed, not as sensations, but as complexes or blends of sensation, "organic perceptions as it were." He draws this conclusion for 2 reasons: first, because his observers report that the pressures resemble complexes of sensation rather than simple qualities; and second, because in the affective situation he was able to get reports of simple bodily pressures, localized like bright and dull pressures, yet reported as distinct from them. This would indicate that the pressure experience was a complex one and might yield to further introspective analysis.

Beebe-Center's acceptance of the sensory nature of bright and dull pressure seems to be conditioned partly by Nafe's interpretation and partly by the fact that it is possible in experimenting upon the hedonic judgment to duplicate the phenomena of contrast and relative judgments which are typical of psychophysics. We have already suggested, however, that such similarities between psychophysics and affectivity may be considered as demonstrating a higher process of "judgment," rather than as indicating that the affective response involves the participation of pressure sense organs of the same sort as those involved in judgments of lifted weights. The demonstration of these common effects in other fields of value judgment (*i.e.* aesthetics) is a future possibility that would go far in answering this question.

One more recent theory on the nature of the affective consciousness must be mentioned. Ruckmick (**56**) assumes the existence of some specific type of experience, and proposes a phylogenetic theory which views hedonic tone as a survival of the primordial undifferentiated consciousness from which the more specific experiences have gradually developed as the organism has evolved phylogenetically. As such, pleasantness and unpleasantness demand no receptor mechanisms, but depend upon "all existing channels for the conveyance of electrochemical energy." The argument is entirely by inference, and there seems little chance for experimental verification. How such a view would relate to any attitudinal theory is not clear. The intangibility of such primordial experience renders its specific relation to the organism a somewhat vague affair.

THE PHYSIOLOGY OF THE AFFECTIVE STATES

When we leave the phenomenological field and look at the work upon the physiological correlates of pleasantness and unpleasantness, we find that little has been added recently. The somewhat desperate earlier attempts to correlate the affective states with some one of the visceral processes seem to have passed out of fashion, no doubt due to the discouragingly negative nature of the results. Wherever such correlates are demonstrable, as in the field of the galvanic skin response (**8**, **35**), the relation is neither clear nor exclusive, and often may be due to the simultaneous presence of emotion. Obermann (**48**) has recently failed to find any regular changes in the electroencephalogram during pleasantness and unpleasantness.

The confused state of the results on the physiological correlates of affection strongly resembles that in the related field of emotion, but this is a cause for hope rather than despair. The recent recognition that the response picture in emotion is a complex one and therefore demands for its study some sort of analytic technique rather than simple observation has begun to yield results. Thus Landis and Hunt (**36**) have revealed the existence of a definite skeletal pattern of response in the startle situation by ultra-rapid photography through the magnification of the temporal dimension of behavior. Gaskill and Cox (**19**) deny the existence of a plain indicator of emotion, but do find that the analysis of variance and covariance indicates the presence of "patterns" in respiration, though these patterns are exceedingly complex. Beebe-Center and Stevens (**3**) have recently pointed out the complex neural basis of cardiac acceleration and have more recently (**4**) noted the influence of skeletal responses on visceral changes. This necessitates a revision of the classical concept of visceral change as a primary reflex response to the emotional stimulus and a recognition that such change may be a secondary result of skeletal response.

The response to hedonic stimulation can be expected to be just as complex. The stimulus in the hedonic situation might be expected to result not only in an affective response of some sort but also in specific, nonaffective responses of varying sorts, and all this would be superposed on the picture of skeletal and organic tonus existing at the moment of stimulation. When you add to this already complex picture of behavior the changes due to interrelationships between these elements, you reach a confusion that might well cause despair, were it not for the progress being made in the equally difficult problem of emotion. If sophisticated analysis yields results in the study of emotion, one has every right to hope it will be equally effective in studying affectivity.

Skinner (**57**) has argued against the interpretation of emotion as containing special varieties of behavior and has substituted the interpretation of emotion as a change in the strength of reflex response. Thus he is interested, not in the fact that a shot may cause a skeletal " startle " response in the hen, but that it may cause a suspension of eating. " The important thing is the recognition of a change in strength as a primary datum and the determination of the functional relationship between the strength and some operation." Except for the specific formulation which Skinner offers, this approach is not a new one in affectivity. It might be argued that the work of Munsterberg and others (2) on the facilitation and inhibition of movement during affection is exactly of this sort. Moreover, if one defines respiration, cardiac activity, etc. as measurable reflex responses and views these, not as initiated by the affective stimulus, but as having their usual activity altered in strength by the hedonic stimulus, one is not only in tune with the recent tendency in studying these phenomena, but certainly very close to Skinner in spirit.

Indeed, one may ask whether the whole experimental investigation of the relation of affectivity to memory and learning is not of this same general type, involving as it does an increment or decrement in behavior, although this behavior admittedly is not of a reflex type. As we have stated above, we will not review here the material on hedonic tone and memory or learning. In passing, we may regret the fact that the influence of affectivity usually has been assumed implicitly to be a matter of some primary factor in the central nervous system, without adequate consideration of the possibility that it might be exercised through such secondary factors as are involved in the differences in attention, effort, coöperation, etc., attendant upon a general bodily attitude of acceptance or rejection.

The work of Cason (10) on the influence of tension and relaxation upon the feelings is interesting and of the sort that may develop suggestive findings. So far, however, he is concerned largely with feeling as emotion rather than as affection in our use of the term. This is also true of his earlier theoretical paper (9), where he points out that interacting patterns of organic response, plus the process of language and conceptualization, may well account for the richness and complexity of the feelings.

As one might expect, thalamic theories of feeling remain popular. Harlow and Stagner (21) have proposed one which claims 4 fundamental dimensions for feeling: pain-unpleasantness, pleasure-pleasantness, excitement, and depression. The anatomical seat of these processes is assumed to lie in the projection areas of the thalamus. It is to be regretted that a theory such as this, primarily subjective and experiential, has not arisen from, or been verified by, any exact introspective study, but is based wholly on inference and deduction from the scattered clinical findings of others (in this case, largely those of Head).

Lashley (37) has shown how dubious the physiological bases of the thalamic theory are. To anyone acquainted with the methodology of introspection, the experiential bases of the theory will appear as equally unsound. In introspection it is necessary to define terms, carefully control the conditions under which the protocols are gathered, and then interpret them with care. Some of our medical clinicians seem to forget this, and gather introspections as casually as they might collect passing opinions on the weather. It is no disrespect to a great neurologist to say that many of the reports of Head's patients are patently unreliable as scientific material and have been subjected to further interpretation for which they are in no way suited.

Allport's old theory that pleasantness depends upon the action of the parasympathetic branch of the autonomic nervous system and unpleasantness upon the sympathetic branch remains popular with psychologists. Recently it has been restated with great sympathy by Ruckmick (56), and Lund (39) accepts it in his new book on emotion. What direct experimental evidence we have seems to indicate that both branches of the autonomic nervous system may be activated in both affective states, but the simplicity of the theory renders it attractive, and it perseveres despite the indications to the contrary.

In concluding this section we must mention a line of experimentation which, while its specific relations to affectivity are not clear at present, holds promise for the future. This is the study of mood. A halfway territory somewhere between affection and emotion, it is usually classed with the latter. Young (66) found that there was no correlation between affective ratings for odors and the rating of the mood. On the other hand, Bousfield and Barry (6) found evidence that mood influenced the production of affectively toned associations—a result which is supported by the work of Johnson (33). An indication of the complexity of the subject is furnished by Young's (66) warning that we must differentiate between immediate reflex responses and those mediated by associative processes aroused by the stimulus. As the work on mood continues [*i.e.* Johnson (34), Bousfield (5), Dysinger (14, 15)], its bearing upon affection should become clearer.

WHAT STIMULI ARE PLEASANT OR UNPLEASANT

The study of what stimuli are pleasant or unpleasant no longer commands the interest it once did. There are many reasons for this. There has been a rapid decline in the popularity of hedonism as a psychological basis of motivation. The large individual differences exhibited have precluded the possibility of any exact prediction of the hedonic value of stimuli for specific individuals. Where rough, statistical generalizations for preference have been established, as in the field of color, these have not led to any further insight concerning the affective response. Finally, the behavioristic trend of the last 2 decades has shifted interest from the introspective approach, with an attendant disregard of experiential contents and an increasing tendency to separate the study of preference as reflected in "choice" behavior from its related affective experiences. While there is no necessary separation between these 2 aspects of the general hedonic problem, such a separation unfortunately has developed in practice.

Sporadic studies of the affective value of stimuli do continue, however, as witnessed by the appearance in the last year of the paper of Garth, Moses, and Anthony (**18**) on the color preferences of East Indians, and St. George's (**58**) study with college students.

Beebe-Center's (**1**) application of Spearman's tetrad difference technique to a series of affective rankings for odors with its resulting evidence for a general hedonic factor has been repeated successfully on colors by Ludvigh (**38**). This reinforces the evidence for such a general factor, but raises the question of whether it is attributable to some potentiality of the stimulus or is a function of associative learning, which possibly might be traced to cultural standards. To deal with this further question Ludvigh had some of his observers repeat their judgments while under hypnosis both without further instruction and after being told not to use association in arriving at their judgments. Under hypnosis, but without instruction, there was still evidence for a general factor; but, when they were hypnotized and told not to use association in arriving at their judgments, the observers changed their rankings and the tetrad equation was no longer satisfied. This would indicate that the general factor was not inherent in the stimuli, but was produced by association. In view of the complex nature of hypnosis, this work had best be repeated carefully before too great a theoretical structure is reared upon it.

We have already mentioned the current shift in stress from studies of affective value to studies of preferential behavior. This development is best seen, perhaps, in the work of P. T. Young, who has undertaken the study of preferential behavior after many investigations of affectivity employing the classical introspective approach. Young (59, 65) is carrying on a series of interesting studies on preferential discrimination ranging from food preferences in rats up to the preferences and aversions of human beings for other human beings. He cautions us that the preferential discrimination, while it frequently involves the hedonic response, cannot be equated to it. Nevertheless, such material has much to offer for the understanding of the affective response, particularly if it is subjected to a thorough analysis. Thus, Maslow's (40) study of the influence of familiarization on preference has many analogies with hedonic experimentation. The Yale work (42) on approach and avoidance also offers many suggestions for hedonic theory.

In mentioning related experimental work in other fields, there is an ever-present danger of going too far. Our analytic categories such as memory, emotion, affection, etc. have served, after all, in making a complex subject intelligible by focusing attention on certain limited aspects of the whole, and it is no doubt best that we rest our survey at this point.

SUMMARY

Our survey has shown us a rich and diverse field which, as yet, certainly is not ready for final unification. Yet the picture is far from chaotic. There is a thread of unity through it all. The main problems are clear, and in many cases the techniques for approaching these problems are at hand. Best of all, the affective response is intimately related to other psychological processes, and its investigation cannot help but shed light on other vital problems in human behavior.

A brief summary would run like this: The organism may either accept or reject a stimulus. This acceptance or rejection is carried out through appropriate bodily adjustments. These reactions are said to constitute the affective response and are assumed to be a functional entity of some kind.

Since the reactions never have been described minutely and carefully analyzed, but have been accepted largely on a basis of rough empirical observation, it is impossible at present to say just what the common core is that unites these responses as the entities, "pleasantness" and "unpleasantness." These concepts may be used in a very general way to refer to some abstract characteristic of the reactions, *i.e.* the behavior in pleasantness is directed " toward " the stimulus and the behavior in unpleasantness is directed " away from " the stimulus; or they may refer to some more concrete manifestation such as the participation of common muscle groups or nervous centers. There is nothing in the words themselves to indicate the level at which the classificatory resemblance exists.

That the relation is a purely logical one is implied by the judgmental theory when it states that all pleasant responses resemble one another, not because of any identical physiological or neurological elements, but because of their common character as behavior which accepts and encourages the stimulus. On the other hand, there may be physiological similarities contributing to the classification.

As Harlow and Stagner (**22**) conclude, the complexities of function in the human organism render it unlikely that we shall find a completely specific and unvarying response as the basic uniformity; yet the evidence for a typical conscious content (which appears prevalent though not universal, and has not been adequately described) seems to point to the participation of some common physiological elements. The possibilities are numerous. The common core might be some skeletal response such as is found in startle. It might be a postural adjustment or a mere increase or decrease in postural tonus. It might be associated with some central nervous phenomenon of facilitation or inhibition. There is the possibility of the involvement of some reflex center such as the thalamus. Whatever it may be, the evidence would seem to point to some physiological as well as purely logical uniformity in the response picture. Moreover, such work as that of Beebe-Center on general affective value would point toward some common characteristic for hedonic stimuli.

All this indicates a basic, innate reaction tendency toward 2 kinds of stimuli. Nevertheless, while pointing out the probability of some innate response, we must remember that learning widens the original possibilities as the organism develops, and that not only may the behavior itself become modified and complicated, but it may wander far from its first stimulus connections. As used by the human adult, pleasantness and unpleasantness are concepts which have a much wider reference than the primary responses on which they originally may have been based. The exact development and use of such concepts and the part they play in motivating the behavior of the individual is not fully understood, but it can be stated that such processes as memory, learning, conditioning, and judgment all participate, and that the hedonic response in the adult is a complicated function of the entire psychological organism and not of any isolated, specific hedonic process.

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BOOK REVIEWS

GUTHRIE, E. R. The psychology of human conflict: the clash of motives within the individual. New York: Harper, 1938. Pp. ix+408.

Certainly, no reader who gets well started on this book will stop until he has finished it. But during the first 200 pages he may experience a kind of baffled wonder as to why *conflict* was invoked in the title. The answer is, of course, that the psychology of conflict is simply a small fraction of the psychology of learning, and hence this textbook on abnormal psychology must be devoted first of all to a detailed presentation of learning theory and the rôle of motivation. Then comes the *conflict,* and bafflled wonder may well turn to "aha!" enthusiasm.

Guthrie has derived the processes of symptom-formation as special cases of various kinds of learning. Further, he has given a behavioristic cast to the whole field of abnormal psychology. As few others have been able to do, Guthrie has thrown into bold relief the conception that much of the behavior which has puzzled psychologists no less than laymen is merely a reflection of ordinary psychological processes against a special background of personal history and social forces. The phobia is only a learned fear, developed and maintained under conditions which do not happen to most people; temper tantrums are angry explosions in children and adults who have had atypical reinforcements and inhibitions from ordinary parents. Character quirks, social reaction patterns, the uniqueness of each personality are but the consequences of normal learning in special environments; these problems refer to the *content* rather than the abstracted *process* of learning.

With this emphatic background of learning theory, the author's acknowledged dependence on Janet's notion of mental energy seems incongruous. "Mental energy" has been interpreted in terms of Cannon's theories of the reinforcement of action, but, in essence, Guthrie relies on an innate property of the organism to account for the development of neurotic, rather than normal, behavior. Both kinds of *behavior* develop as products of the same laws of learning, but neurotic and normal *persons* differ in the amount of "mental energy" (adequacy of reinforcement of action) they possess and the kinds of social living to which they are subjected. No one can quarrel with the importance accorded the latter factor, but the proponent of innate weakness as an explanation of hysteria must surely meet the criticism that in many clinical cases the disordered behavior is apparently eliminated by extensive reëducation. In other words, the alleged native disability disappears under the same conditions that remove learned disabilities. This criticism obviously does not invalidate Professor Guthrie's argument, but it is one which requires answer.

As a text for courses in personality, mental hygiene, and abnormal psychology, the book may prove a little difficult. This will appear paradoxical on first examination, since the style is light and the evidence largely anecdotal. The arguments are subtle, however, and the student may be seduced into a too casual and superficial reading.

ROBERT R. SEARS.

Yale University.

DORCUS, R. M., & SHAFFER, G. W. Textbook of abnormal psychology. (2nd ed.) Baltimore: Williams & Wilkins, 1939. Pp. xviii+475.

Except for the inclusion of a brief chapter on chemical therapy, the second edition of this book differs but little from the first. There are minor additions and rearrangements in the text, and the results of some recent researches have been incorporated in various parts of the discussion. What was said in a review of the first edition may, in the main, be repeated for this one.

There are various virtues to be noted. The descriptions of symptomatology are thorough and complete. References to psychiatric and semipsychiatric literature are extensive. There is a minimum of emphasis on classificatory schemata for mental disorders. Speech disorders are discussed more adequately than in any other text, and the final chapter on therapy remains a useful teaching guide.

On the debit side, however, certain characteristics of the authors' viewpoint seem even less desirable today than they did five years ago. The book is descriptive rather than dynamic, physiological rather than psychological. Throughout there is an appeal to a kind of specious neurologizing, a lugging in of polysyllabic physiology which is not always warranted by contemporary knowledge in those fields. For example: "Theoretically, amnesia due to concussion represents failure of retention. The disturbed circulation prevents retention" (p. 122). What is the evidence? Disturbances of circulation produced by massive cranial operations do not customarily produce amnesia.

The new section on disorders of memory is of no very evident advantage to either teachability or scholarship. It is primarily a description of symptoms. In fact, throughout the book there is a minimum of psychology; and what there is does not have a very modern ring. The section on "amatory desire," for example, ignores nearly all the mass of information concerning sex behavior which has been collected during the last fifty years. Other omissions are equally conspicuous. The reader will search in vain for any indication that the tremendously significant advances of the last five years in the fields of personality and learning have influenced the new edition. And it is with something of a shock that one discovers that there is no mention anywhere of G. V. Hamilton, whose contributions to psychopathology are possibly the most important that have come from any American investigator.

If the purpose of the book is to provide a compendium of symptoms and description of disordered behavior, it succeeds; if any reader wishes to know something of the life-history origins or psychological factors involved in mental disorders, he will have to look elsewhere.

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ROBERT R. SEARS.

Witty, P. A., & Skinner, C. E. (Ed.) Mental hygiene in modern education. New York: Farrar & Rinehart, 1939. Pp. x+539.

This book is a compilation of contributions by nineteen authors in the field of mental hygiene. These contributors were selected so as to cover the entire range of workers: experts in the research laboratory; psychiatrists in charge of children's clinics; educators in the classroom situation; and psychologists in the clinic. The book is divided into two parts. The first part deals with the general problem of mental hygiene in the development of personality, and the second part deals with the special application of mental hygiene techniques to mental problems. It is intended to be used by students of education and psychology, teachers in service, school supervisors, administrators, adult education groups, and parents. In addition, the authors hope that it will be of interest to all socially-minded persons who hope to foster a "good life" in American children.

In the "Introduction" the authors point out that schools are still primarily concerned with the conformity of children to "academic requirements rather than with the development of flexible minds able to cope with new or troublesome situations." Instead of helping youth to meet its present-day problems, schools actually very often contribute to children's puzzlement and maladjustment. In the past there were sufficient means for youth to attain emotional maturity despite the schools' failure to prepare them adequately to meet life's emergencies. Today, with the prolongation of dependency, it is becoming more difficult for youth to adjust without adequate guidance. The authors have attempted in this book to present the small, but reliable, body of knowledge concerning personality development which can be useful in the classroom and in the home to guide the development of the young.

The first part of the book includes chapters on personal development of the child from early infancy up through adolescence and an additional chapter on adult education. In this section the contributions of Harold H. Anderson and John N. Washburne are particularly useful. Anderson, unlike some of the other contributors, brings to his discussion the assistance of psychiatric concepts in the area of security that can be added so well to the tools of the psychologist. The rôle of education as a potential pathogenic agent, especially at the time of entering school, is stressed by several of the writers. The requirements of a healthful democratic environment necessary for mental hygiene are contrasted with the unhygienic nature of the perverse pattern of American culture, with its tendency to prevent individuals from thinking for themselves and from accepting new ideas. The personality development of the adolescent is described by John N. Washburne in a chapter which includes a discussion of the problems of nature *versus* nurture, individual difference, types and modern methods of measurement. He points out that, although typology has been, and still is, to some extent in disrepute, a practical need for type classification exists today. Administrators today accept whatever type classification is now available. For example, the feebleminded are treated as if they constituted a single type and are often given special environment on the basis of low intellectual status alone. There is need today for a more comprehensive typology—and the author sponsors the need for measures of individual similarity in contrast with the over-developed methods of individual differences.

The second part of the book devotes itself to special problems, among which are included educational diagnosis and remedial instruction, personality development of the physically and mentally handicapped, and the rôle of the teacher and classroom in personality adjustment. Outstanding among these contributions are Louttit's on personality disorders and Watson's on the rôle of the teacher. Louttit classifies the various types of personality disorders with special reference to their detection and diagnosis. Watson points out the importance of the teacher's own mental health in her attempts to influence child behavior and development.

In common with most other books on mental hygiene this book promises more than it can possibly fulfill. The critical reader cannot help but feel that the amount of scientific knowledge in the field of mental hygiene is very limited, despite the attempts of well-wishers and idealistic educators to promote the "good life" under the aegis of the mental hygiene movement.

The subject matter that the book covers, ranging as it does over the entire field of human development, makes the task of the reviewer a very difficult one. Because of its broad scope the book does not cover each of the topics evenly. Some of the chapters are models of conciseness and carefully evaluated summaries of extant research. Others are more loosely written and with less care for the controls that differentiate general observation from scientific results. For example, the statement that "four-year-olds more than other children, tend to criticize others and to seek attention and praise" may or may not be correct, depending on what four-year-olds were observed and how the observations were made. The unfortunate result of such statements, when placed in the hands of students, is either to discredit psychology, and especially mental hygiene, or to create a dogmatic belief which is quite in contrast with the attempt to develop flexible minds referred to in the "Introduction."

Another shortcoming that the book suffers from is the failure to include reference to the work of psychoanalysts, especially in the chapters dealing with the emotional development of the child and the adolescent. No matter how unscientific some of the work of the analysts may be, their basic concepts are now part of the working hypotheses of nearly all students of behavior.

Psychologists may find it very difficult to accept Stevenson's ex-cathedra pronouncement that administration of clinics must invariably be lodged in the hands of psychiatrists. The primary reason for this arrangement is stated to be the fact that the clinic entails medical responsibilities for which there are usually legally defined obligations. Many psychologists may differ with him on this point. When the problem before the clinic is primarily an educational problem, as it often is, it is at least questionable whether the administrative function should be in the hands of the medically trained psychiatrists rather than in those of the educationally trained psychologist. Furthermore, in some instances the full responsibility might well lie with the psychiatric social worker, and it is reasonable to suppose, in clinics where the social worker possesses the necessary administrative ability, that she be permitted to be the administrative head. The many child guidance clinics that are now administered successfully by psychologists and by social workers would tend to offer substantial validity to this claim.

Another of the book's difficulties emanates from its organization. Because of its attempt to treat separately the various levels of development much unnecessary repetition is introduced. Despite these difficulties, the book does present a very readable account of the available knowledge in the field of mental hygiene. It can serve as an interesting textbook in mental hygiene, provided that satisfactory supplementary readings are included.

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SALISBURY, F. S. Human development and learning: an interpretive introduction to psychology. New York: McGraw-Hill, 1939. Pp. xvii+513.

The subtitle to this book, *An interpretive introduction to psychology,* is a bit misleading. The author has not attempted an introduction to psychology so much as an interpretation of the prospective student's everyday problems in the light of psychological findings. The book is evidently written for the prospective elementary and secondary teacher. The author has taken as his task the problem of selecting and interpreting those psychological materials and topics which will be of greatest immediate assistance in the teaching situation.

In harmony with this purpose there is a noticeable absence of controversial theoretical issues. No time is spent on the implications of different points of view for educational practice, and there are no detailed accounts of experimental and laboratory methods. The book is not, and makes no claim to being, theoretically systematic, although the treatment is colored throughout with a bias toward *Gestalttheorie.*

The author employs the genetic approach to all topics considered. The student is asked to keep clearly before himself the fact that the present stage of development can be interpreted only in terms of the earlier stages. To assist toward this attitude the second chapter is devoted to a description of the " Nelson " family in which the different developmental stages of the three children are pictured in contrast with each other and with their parents. Chapters 3 to 6, inclusive, deal with embryological development, with the development of the control of behavior through the nervous and muscular systems, and with the rôle of the autonomic and glandular systems in emotional development. Chapters 7 to 12, inclusive, deal with the development of intelligence, the learning process, memory (styled " Conservation of Experience "), and thinking. The remaining chapters, 13 to 18, are devoted to problems of individual differences and personality.

His treatment of learning will serve to illustrate in some more concrete fashion just how the author proceeds to show the bearings of psychological materials on the interpretation of the problems presented the teacher by the school child. The topic is covered in three chapters: " Organization in Learning and in Conscious Experience"; "Thinking and Learning Take Direction"; and "Factors That Influence Learning." In the first the main problem seems to be that of trying to discover how the external world is perceived by the child. Learning is pictured as a developing, dynamic process that means continuous changes in the external world as well as in the learner. To appreciate just how things appear to the child we are reminded of the experiments of Köhler, and of Wheeler and Perkins, which demonstrated that children, hens, chimpanzees, and gold fish made "relational" choices. These experiments show that experiences are organized into patterns. Meanings arise with this patterning. Repetition (the experiments of Thorndike) plays no rôle in the development of meanings. Likewise, contiguity alone is powerless without the strengthening function of belongingness and acceptability (Thorndike's work again). The author holds that all of this demonstrates that the factors that "count in learning are those that bring about organization in the experience." Some newer educational procedures are then described as being illustrative of this newer view of learning. Lashley's studies of brain function are also introduced to support the contention of the primary and basic rôle of organization in learning.

The conditions giving direction to learning and thinking are described under the rubric of interests, not motivation. (The terms "motive" and "motivation" appear nowhere in the text.) The genetic history of interests is traced from the first dim awareness of babyhood through the development of the concepts of the self and the not-self, together with concepts of the relational and dynamic aspects of these two. These concepts then become patterned into larger wholes called "interests." This development of the topic leads into a discussion of the nature and value of vocational interest tests.

On the problem of the factors influencing learning the author lays most stress on the rôle of maturation in permitting the development of insight. The traditional long list of factors influencing learning is not mentioned, and only cursory treatment is given trial and error learning and the traditional laws of learning.

This description of his treatment of learning will give a rough notion of his treatment of other topics. The more technical psychologist will not approve of the text because of its lack of experimental detail and the manner in which the author departs from the conventional psychological categories. The reviewer does not feel qualified to say whether a book of this sort will be of real assistance to the prospective teacher, or, rather, whether it will be of greater aid than the more conventional type. The book is written in an easy, fluent style that will probably hold the interest of the students for which it was written.

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AUGIER, E. La mémoire et la vie: essai de défense du mécanisme psychologique. Paris: Félix Alcan, 1939. Pp. 251.

The title of this book, although not seriously misleading, does not indicate the thesis which its distinguished author has sought to develop. In earlier works *(De l'action à la connaissance,* 1923; *Une psychologie objec-*

tive est-elle possible?, 1928; *Mécanismes et conscience*, 1934), M. Augier presented a Gallic version of American behaviorism. In the present volume, which includes more than memory and considerably less than life, the author returns to the same thesis, but broadens the bases of his arguments to such an extent that the whole range of mental phenomena, from tropisms to insights, is brought within the scope of psychological objectivism. The negativism and dogmatic exclusions of early uncritical behaviorism have disappeared, to be replaced by a tolerant doctrine which argues that all psychological data are capable of formulation in objective terminology.

The gist of Augier's argument is that all concepts, if they are to have any scientific value, must be couched in language which makes use of spatial and temporal coördinates. His use of the word 'objective' (unfortunately nowhere explicitly defined) is always in relation to the language of concepts, never to the data of observation. Initial data, in and of themselves, are neither objective nor subjective, except possibly in some metaphysical sense which is of no concern to science. For science data are merely what they are, which is to say that they are of no intrinsic importance. A datum acquires scientific significance only when it is observed in relation to some other datum, and then it is the relation, rather than the end-terms, that is important.

Experimentally determined relations between data are the starting points of scientific concepts, and these relations are universally described in the natural sciences in terms of space and time. But the data of natural science do not differ in kind from those reported by so-called introspection. Indeed, the initial data of all sciences are introspections. It must therefore follow that psychological concepts, regardless of the data upon which they are based, need be no less objective than the concepts of physical science, both because their data are indistinguishable from those of other sciences and because objectivity of concepts is a matter of logic, not a matter of fact.

The position which Augier defends is strikingly similar to that of the logical positivists and the operationists. The character of any given science is revealed by the language and logic of its concepts, not by its initial data of observation. The subject matter of all sciences, including psychology, is the same, so that all efforts to define psychology in terms of some unique mental stuff are futile and represent the lingering remains of a rapidly vanishing metaphysics. There is no evidence that Augier has been directly influenced by the writings of those scientists and logicians who at the moment so vigorously espouse the cause of unity of science, yet every paragraph of the present volume is permeated by the spirit of this newest hope in the logic and methodology of science.

CARROLL C. PRATT.

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LUCK, J. M. (Ed.), & HALL, V. E. (Assoc. Ed.) Annual review of physiology. (Vol. I.) Stanford Univ.: American Physiological Society & Annual Reviews, 1939. Pp. vii+705.

This is the first volume of an annual review intended "to provide the reader with a comprehensive survey of the year's research in physiology."

It will differ from other reviews such as *Physiological Reviews,* which "provide exhaustive treatments of selected topics," in that it will give broad, year-by-year résumés of the significant work in the entire field of physiology.

A comparison of this volume with the 1938 volume of *Physiological Reviews* shows some resemblances of treatment in the two, the main difference seeming to be in the limitation of references to about one year, though not always the calendar year, 1938. So far as breadth of the survey is concerned, Kerridge's "Recent advances in knowledge concerning hearing and speech" in *Physiological Reviews* is as broad as those in the "Annual Review." However, there is no overlapping of fields in the two volumes.

It is expected that the list of topics treated and the manner of treatment will vary from year to year as the emphasis in research output shifts. The distinguished editorial committee may be counted on to produce an important document, regardless of the particular method and content of any given year.

This first volume contains material all of which will be of interest to many psychologists. It provides easy access to a surprisingly large number of references in foreign languages. Each of twenty-four topics is reviewed by a well-known specialist in the field. Of these topics ten should be of special psychological interest. They are: "Physiological Aspects of Genetics" (G. W. Beadle); "Developmental Physiology" (Joseph Needham); "Energy Metabolism" (J. R. Murlin); "Electrical Phenomena of the Brain and Spinal Cord" (Hallowell Davis); "The Spinal Cord and Reflex Action" (J. C. Eccles); "Bioelectric Studies of the Excitation and Response of Nerve" (D. W. Bronk and F. Brink, Jr.); "The Autonomic Nervous System" (J. C. Hinsey); "The Special Senses" (J. M. D. Olmstead); "Physiological Psychology" (E. R. Hilgard and C. P. Stone); and "Endocrine Glands" (H. M. Evans).

The article on "Physiological Psychology" is divided into two parts: "The Conditioned Reflex" (Hilgard) and "Miscellaneous Topics" (Stone). Hilgard considers it a desirable practice "to reserve the name conditioned reflex for alterations of response in experiments following the prototype of Pavlov and Bekhterev." This, perhaps, provides the justification for including the conditioned reflex under physiological psychology, since it excludes that kind of learning recently called the "Thorndikian response." Still, it is not easy to see that one kind of learning is more physiological than the other, except in terms of the origin of the techniques.

"Miscellaneous Topics" includes Development of Behavior, Brain Mechanisms, Behavior Related to Nutrition, Studies Bearing on Drive and Motivation (including Drive, Energy Output, and Sex Behavior), Factors Related to Mental Work and Efficiency (including Anoxemia, Drugs, and Bodily Tensions). Only the high lights in these topics are presented, with fifty-eight references covering all of them.

The reviewer has only one criticism to make of the survey of this enormous mass of material. It is that, in a volume which is essentially a reference work, the citations should be more systematically arranged.

Each author apparently used his own taste in arranging his bibliography. The best are arranged alphabetically and numbered, and are referred to by number in the text. Others are neither numbered nor alphabetically arranged, and in several instances the references by the same author are not grouped together, but are scattered through the list according to the order in which they are referred to in the text. This lack of uniformity in organization will, doubtless, be corrected in the succeeding volume.

A. T. Poffenberger.

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Moore, H. Psychology for business and industry. New York: McGraw-Hill, 1939. Pp. xi+527.

Although the title of this book would indicate that it is aimed at those actually engaged in business and industry, the Preface states explicitly that it seeks, rather, "to introduce the student to those aspects of personnel problems in business and industry to which psychology has given, or can give, a contribution." The author reports: "I . . . was constantly disturbed by the contrast between the results from laboratory experiments and the reported experiences of adult members of my classes." The result of this dilemma is a book in which the author has endeavored to marry the "practical" with the scientific.

The first chapter consists of a sketch of psychology's service to business and industry. A chapter on "Getting a Job" follows. Chapters III to IX, inclusive, deal with the importance and methods of fitting the worker to the job. Chapters X to XIII treat motivation, employee accidents, fatigue, and the problem employee. The last two chapters are concerned with psychological problems in advertising and selling. The volume as a whole is essentially a review of topics in the psychology of industrial personnel rather than the more general approach which the title might lead one to expect.

The book is replete with evidences either of haste or of carelessness. For example, the reader is introduced to the "Psychology Corporation" upon at least four occasions, and there are references to "*V*. V. Bingham," "C. H. Griffit*h*s," "H. H. Borden," and "Burtt, H. E., Employment Psychology, . . . *1936*." On page 58, the table is not self-consistent, while the table on page 322 does not tally with the descriptive text. There will be those who will scarcely approve the recommendation of Dick Carlson's *How to develop personal power* or the citations from popular periodicals. This reviewer was astonished to learn that "personality is the most important contribution anyone can carry to an organization" (p. 46), since the author had confessed, only two pages earlier, that personality "defies description."

The value of the volume is not enhanced by a looseness of statement which pervades its pages and of which the following citations are typical:

> "The guide should follow up the development of the habit and acquisition of the necessary skills so that the beginner functions *at his maximum efficiency—his physiological limit*" (p. 249).

"Repeated surveys of the Psychology Corporation (*sic*) have shown that competitors of an advertiser *benefit more from his advertising than he does*" (p. 452).

"This is due primarily to the fact that *most people are visual-minded* and can retain an image of a picture much more easily than they can an image coming from any other sense organ" (p. 460).

"The Advertising Clubs of the World . . . have succeeded in eliminating dishonest, and much objectionable, advertising" (p. 450).

(Italics are the reviewer's.)

Thoughtful individuals will not question a psychologist's right to be practical; however, they may well question his right to label practicality as psychology. An inclination in the latter direction tends to weaken the genuine service Moore has rendered in assembling a wealth of material from American and European sources. Not even this, however, can prevent the volume from becoming a useful source of reference for the cautious teacher.

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RIDER, P. R. An introduction to modern statistical methods. New York: Wiley, 1939. Pp. ix+220.

For review purposes, the contents of this book may, perhaps, be profitably divided into two parts, to be considered separately. The first five chapters present the basic concepts of statistical method. These chapters deal with the usual textbook material, *i.e.* characteristics of the frequency distribution, methods of graphical representation, measures of central tendency and their respective uses, variability, regression, and correlation, the binomial theorem and its application to probability, the normal distribution and tests of significance based upon it. In the treatment of these topics, several points neglected by many textbooks in statistics are brought out. The close relationship between the binomial and normal distributions, for example, is clearly explained. Also, the relative nature of the concept of significance is made clear, and R. A. Fisher's concept of "fiducial," or confidence, limits is introduced. In evaluating the significance of a correlation coefficient and of the difference between two coefficients, Rider discards the probable error (following Fisher) in favor of a logarithmic transformation, which is more precise when one is dealing with small samples. The nomenclature followed throughout the text is that used by Fisher.

From the point of view of its usefulness to psychologists, this first part of the text probably does not fulfill the author's claim, made in the Preface, that his book "is suitable as a textbook for a first course in the subject." The book assumes a knowledge of mathematics which many students, beginning graduate work in psychology, do not possess. Moreover, the author goes into the mathematical derivation of formulas with greater thoroughness than is probably necessary for the student of psychology who is interested mainly in experimental applications. For more advanced students, however, this portion of Rider's book is decidedly valuable.

Rider's chief contribution is in the last four chapters of his book, which consist, in the main, of an exposition of Fisher's methods. The applicability of these techniques to many problems in experimental psychology has come to be increasingly realized, but the difficulty experienced by many students in following Fisher's treatment has served to restrict their use of his methods. Among the topics discussed by Rider are tests of significance for small-sample statistics, *i.e.* the "t" ratio, the chi-square distribution and its uses, elementary theorems in the analysis of variance and of covariance, and, finally, the problem of experimental design. Included under the last heading are explanations of the randomized block method, of the experimental arrangement known as the Latin Square, and of the method of factorial design with its control techniques of confounding and the use of dummy treatments. A knowledge of these methods, as outlined by Rider, will make available to the experimental psychologist, as well as to the scientist in other fields, more precise ways of setting up his experiment and of interpreting his results. As an introduction and interlinear to Fisher, Rider's book is distinctly useful to the researcher in psychology.

HENRY E. GARRETT.

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NOTES AND NEWS

Dr. Frank N. Freeman has been appointed dean of the School of Education of the University of California, succeeding Dr. W. W. Kemp. Dr. Freeman was formerly professor of educational psychology at the University of Chicago.

Dr. Bronson Price, instructor in psychology at the Ohio State University, who was a passenger on the Athenia, has arrived safely in this country.

Dr. Sigmund Freud died in London on September 23 of a heart ailment.

Dr. George F. Arps, dean of the Graduate School at the Ohio State University, died at University Hospital in Columbus on September 16, 1939.

The annual meetings of the Eastern Psychological Association will be held on April 5 and 6, 1940, at Chalfonte-Haddon Hall, Atlantic City. Copies of the Revised By-Laws, to be submitted for final action at the spring meetings, are now available from the Secretary-Treasurer, Dr. Harry Helson, Bryn Mawr College, Bryn Mawr, Pennsylvania. Requests for copies and comments and suggestions should be sent to him. It is desired to frame a set of By-Laws which shall express the desires and meet the needs of as many members of the Association as possible, and suggestions will be welcomed by the Board of Directors. Members are urged to send changes of address to the Treasurer of the American Psychological Association by December 15 in order to insure correct addresses on material regarding the Atlantic City meetings, as the addressograph of the parent organization will be used in sending out notices.

The staff of the new Rochester Guidance Center, Rochester, New York, a mental hygiene clinic for Rochester and Monroe County, financed jointly by the Community Chest and the County, has now been made complete. The full staff of the Center is as follows: director: Carl R. Rogers, Ph.D.; psychiatrist: Chester L. Reynolds, M.D.; clinical psychologists: Gordon L. Riley, M.A.; Virginia W. Lewis, M.A.; Chester C. Bennett, Ph.D.; Annette L. Gillette, Ph.D.; Natalie A. King; psychiatric social worker: D. Elizabeth Davis, M.A., M.S.W.; intake secretary: Mrs. Constance Obourn, M.A.; psychological interne: Mrs. Dorothy I. Wronker, M.A.

A variety of problems covering infancy to age 21 will be accepted at the Center for study and treatment. The organization is unique in that its work is more closely integrated with that of other community agencies than is usually the case with child guidance clinics; and it frankly recognizes the overlapping function of psychiatrist, psychologist, and psychiatric social worker.

VOLUME 1, Number 1, of the new *Journal of Criminal Psychopathology,* under the editorship of Dr. V. C. Branham, appeared in July of this year. The journal is published by the Woodbourne Institution for Defective Delinquents, New York State Department of Correction, is issued quarterly, and is distributed free of charge as a project of the Institution's class of journalism. Original articles, abstracts from current literature, and book reviews are included. All communications with regard to the journal should be addressed to Dr. V. C. Branham, Woodbourne, New York.

NONRECEIPT by a subscriber of any European scientific journal seriously needed as research material should be promptly reported to the American Documentation Institute. The Cultural Relations Committee of the Institute is working closely with the Cultural Relations Division of the Department of State in an effort to bring it about that all research material having no relation to war, regardless of origin or destination, shall continue to pass freely. Reports, with full details as to where subscription was placed, name and address of subscriber, volume, date, and number of last issue received, should be addressed to the American Documentation Institute, Bibliofilm Service, U. S. Department of Agriculture Library, Washington, D. C.

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